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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

Vol. XII 1960 Part 1

THE INFORMATION-CAPACITY OF THE HUMAN MOTOR-SYSTEM IN PURSUIT TRACKING

BY

E. R. F. W. CROSSMAN

From the Department of Psychology, Reading University

Human performance at a typical pursuit tracking task has been analysed by information theory. Without preview of the course, the channel-capacity was found to be about 4 bits per sec. With preview performance ceased to be perfect at about 2 bits per sec. but continued to improve up to at least 8 bits per sec., at about half the ideal slope. The findings are discussed in relation to the information-capacities measured in other tasks, and it is suggested that with full preview the rate should reach a maximum of about 10 bits per sec. Without preview the rate appears to be lowered by the need to offset response lag by predicting course behaviour.

Introduction

The usual experimental pursuit-tracking task consists of a display with a pointer which moves from side to side providing a course or input, $i(t)$. The subject has a control connected to a controlled member, a pointer or pen, which produces a track or output, $o(t)$, and his task is to keep the controlled member aligned with the course pointer so that the error, $e(t)$, is a minimum where

$$e(t) = o(t) - i(t) \qquad (1)$$

In engineering terms the subject is then part of a closed-loop control system or servomechanism whose function is to "follow-up" the course-pointer. There is a well-developed mathematical theory of linear servomechanisms and one of its central concepts is the *transfer-function*, which gives $o(t)$ as a function of $e(t)$ and its differential coefficients; hence several workers have attempted to specify the human transfer-function and they have had some success (Hick and Bates, 1951; North, 1950). Others (e.g. Fitts, Noble and Warren, 1955) thinking along similar lines have measured subjects' "frequency-response" and "phase-lag" when following sinusoidal inputs. However, this approach does not take account of the fact that the human subject has the ability, not shared by engineering control devices, to learn the pattern of the course and hence predict ahead, a fact which has been demonstrated by Poulton (1952, 1957) and called by him "perceptual anticipation." Nor does it allow for his ability to behave at will as an open-chain controller, by attending to the course itself rather than to the error, or to select certain parts of the course for special attention.

The latter aspects of performance can only be taken into account by considering the *statistical* rather than the functional properties of input, output and error, a step suggested by North (1950) but not followed up experimentally. Information-theory is the natural tool to choose for this purpose, and one is led to ask, not what is the output as a function of the error, but at what rate is information transmitted from

input to output, as a function of the input entropy. The important quantities to study are not then the actual functions $i(t)$, $o(t)$ and $e(t)$, but the time-averaged distributions of their values, $p(i)$, $p(o)$ and $p(i,o)$, and the entropies $H(i)$, $H(o)$ and $H(i,o)$ which measure their predictability. The subject's performance is measured by the rate R at which he can accept information from the course and reproduce it as track where—

$$R = H(i) + H(o) - H(i,o) \quad .. \quad .. \quad .. \quad (2)$$

The quantities in equation (2), being parameters of a continuous signal or ensemble of signals, must be handled according to the theory of continuous information, which differs in important respects from the discrete theory hitherto chiefly used in psychology.

The Theory of Continuous Information

(*a*) *The dependence of information-rate on the choice of units.*

In a discrete information-system the entropy of a signal depends only on the relative frequencies with which its different states occur, but in a continuous system it depends also on the size of the step, unit, grouping interval, or quantum used to measure it, increasing with the smallness of this unit (see Shannon and Weaver, 1949, Chap. III). However, provided that all the entropies are computed in the same terms and that the unit is small compared to the finest detail in the probability-distribution of the variable being considered, the rate is independent of it.

If the unit or grouping interval is too large, genuine fine-structure may be blurred out and the rate will be underestimated. But if it is too small, a large sample of performance is needed to give a good estimate of the probability-distribution. If the distribution is estimated from a small sample, chance differences of frequency due to sampling error appear as spurious fine detail and the rate is overestimated. The size of this bias has been calculated (Draper, 1954) and a correction can be applied, but the choice of unit must still be a compromise between the two conflicting requirements. For a given size of unit the rate has a definite maximum, equal to the input entropy.

(*b*) *Time-sampling*

The theory of continuous information sets a limit to the fineness with which the time-scale need be divided up. Since variables in the physical world cannot change instantaneously, their successive values at short time-intervals are highly correlated, which reduces the average information per value; and it has been shown (Shannon and Weaver, 1948, p. 53) that all the information contained in a given variable can be extracted by sampling it at least twice per period of the highest frequency W it can contain (i.e. its bandwidth), that is at intervals Δ, where

$$\Delta = \frac{1}{2W} \quad .. \quad .. \quad .. \quad .. \quad .. \quad (3)$$

(*c*) *Distortion and noise*

A distinction is drawn in the theory between two possible sources of discrepancy between the input and output signals in a channel, namely *distortion* and *noise*, and only the latter is considered as a loss of information. Distortion is any discrepancy due to a known systematic effect, and it can in principle be corrected at the receiving end without loss of information; the remainder, due to unpredictable effects, is noise which does cause loss of information. Distortion arose in the present experiments from delay in the subject's response and from a loss of amplitude at high speeds, and both were corrected before calculating the rate.

These and other theoretical considerations indicate that a tracking experiment must fulfil certain conditions if the calculated information-rate is to be meaningful. First, the course and the subject's performance must both be statistically "stationary" during a trial, so that the averaging processes shall be valid. Second, the successive samples of the course must be, or appear to the subject to be, uncorrelated, so that the calculated input entropy shall fairly represent what he experiences. Third, the bandwidths of course and track must be known and similar, so that the proper sampling interval for both can be

calculated from equation (3). Having obtained a record of the subject's performance the following steps must then be taken to obtain his information-rate:—

(1) Measure the values of corresponding sample-points of course and track at time-intervals Δ chosen according to the bandwidth.

(2) Compute the frequency-distributions of the sample values of course and track and their joint distribution, with a properly chosen grouping interval, allowing if necessary for distortion.

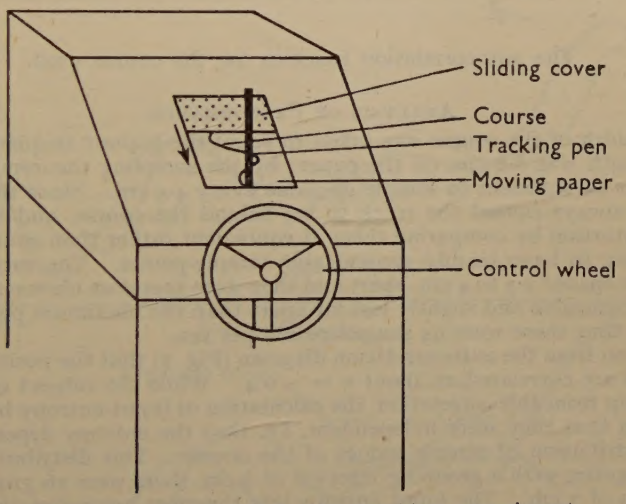
(3) Compute the input, output and joint entropies per sample-point.

(4) Compute the rate per sample from equation (2), and hence the rate per sec.

Experimental Method

The apparatus used for the present experiments is illustrated in Figure 1. The main unit consisted of a paper-drive and course-generator driven by a variable speed velodyne motor, and the control was an 18 in. vertical hand-wheel geared directly to a laterally moving pen, the controlled member. The subject stood facing a window within which he could see the course drawn by a hidden pen in black ink on a band of paper 8·5 cm. wide and his own pen made a red line. The amount of the course visible above his pen, the *preview*, could be reduced by a sliding blind; both course and track were visible for a short distance below the subject's pen. The hand-wheel had to be moved through about 40° to cover the full width of the track and the subject's hand-movements thus extended about 6 in. up or down in a slight curve; the wheel offered little resistance. For a trial the motor was started by a switch after a verbal warning, and the paper then ran at set speed until one cycle of the course-generator had been completed, when it stopped automatically. Eight speeds were used; they are given in Table III.

FIGURE 1



The apparatus used in the tracking experiment.

The subjects were instructed to do their best to keep the pen-point on the course, but that if unable to follow all its variations they were to attempt to reproduce it even if late. Four subjects were tested in the main experiment; each was given two practice runs, then one experimental run at each of the eight speeds with short ($\frac{1}{2}$ cm.) and long (8 cm.) preview, making 16 runs in all, the conditions being taken in random order. For the first two subjects the course was a pair of black lines 1 cm. apart, and for the second two a single line. The time-variation of the course was generated by a pair of epicyclic gears driven from the same shaft as the paper-drive, so that the course had the same shape on the paper at any speed. The gears had ratios of 5/3 and 3/4 and slightly different radii, the course function being:—

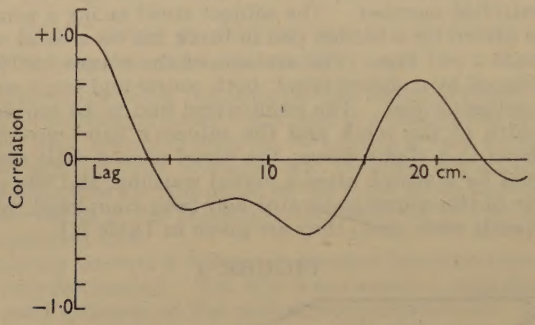
$$i(x) = 1{\cdot}25 \sin x + 1{\cdot}10 \sin \frac{8}{3} x + 1{\cdot}27 \sin \frac{17}{12} x$$

where x = distance along the paper in units of 3·8 cm.

The h.c.f. of the three frequency-components in the course is $\frac{1}{12}$ and it therefore repeated after respectively 12, 32 and 17 cycles. The apparatus automatically stopped after one such repetition, which constituted a run. There were 94 peaks (reversals of direction) during one run.

The course was intended to be an approximation to a band-limited random signal, in that the subjects were not supposed to be able to predict ahead by learning the pattern. Its autocorrelation-function, which is very similar to a part of the course itself, is given in Figure 2, and it can be seen that the maximum correlation in the first 25 cm. is $r = +0{\cdot}64$; r does not reach unity until at the end of one complete cycle, so that subjects cannot in principle improve their performance greatly except by learning the course as a whole, which did not appear to happen. It was therefore considered reasonable to regard the course as unpredictable over any range greater than 2 cm.

FIGURE 2



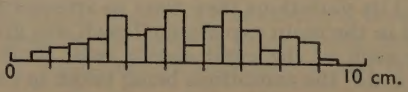
The autocorrelation function for the course used.

Analysis of Performance

The bandwidth of the course was taken to equal the highest frequency present in it, whose wavelength was 8·8 cm. on the paper; by the sampling theorem, equation (3), it would therefore be sufficient to sample its value every 4·4 cm. Since the subject's finite response-time always caused the track to lag behind the course, and it was desired to correct this distortion by comparing them at equivalent rather than simultaneous points, it was necessary to have readily recognizable sample-points. The successive peaks on the course were spaced 2·5 to 4 cm. apart and they were therefore chosen as sample-points, being both recognizable and slightly less far apart than the maximum permissible according to theory; thus there were 94 sample-points per run.

It can be seen from the autocorrelation diagram (Fig. 2) that the positions of successve course samples are correlated at about $r = -0{\cdot}4$. While the subject could in principle derive some help from this correlation, the calculation of input entropy has been based on the assumption that they were independent, i.e. that the entropy depended only on the probability distribution of sample values of the course. This distribution (Fig. 3) was roughly rectangular; with a grouping interval of $\frac{1}{2}$ cm. there were 16 groups in the maximum excursion of 7 cm. The input entropy was therefore approximately $\log_2 16$ or 4·00

FIGURE 3

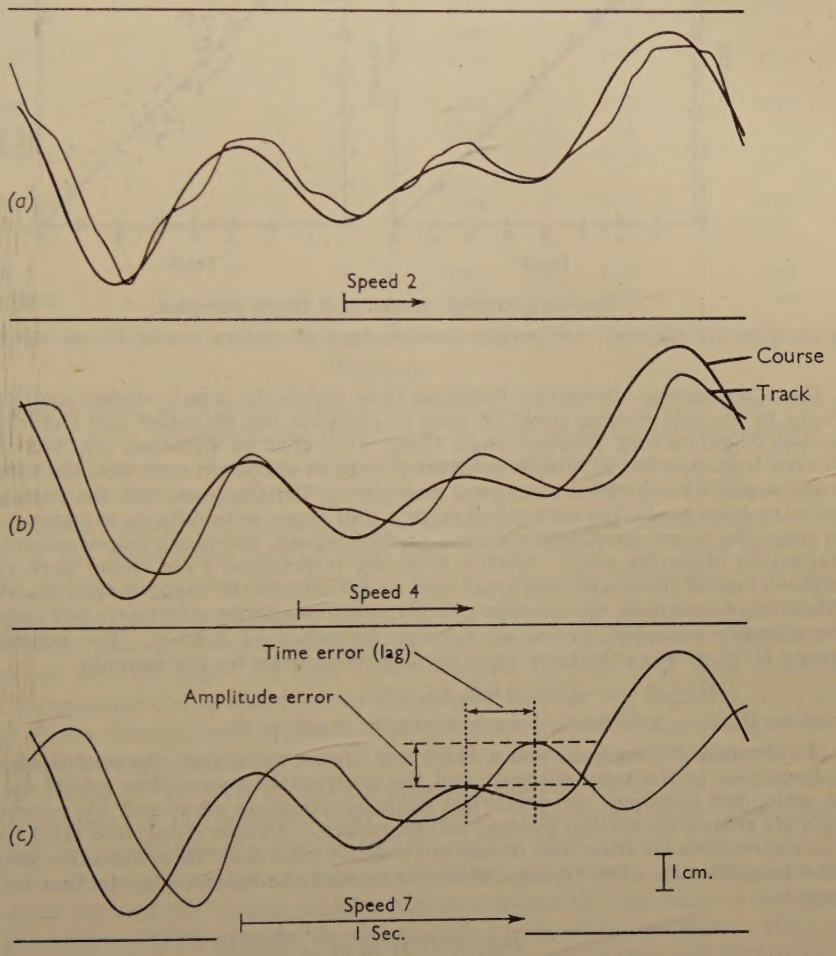


The amplitude distribution of course samples. (94 samples; grouping interval $\frac{1}{2}$ cm.).

bits per sample, though the value calculated from the actual distribution is 3·91 bits. The choice of $\frac{1}{2}$ cm. as grouping interval is equivalent to a choice of $\pm\frac{1}{4}$ cm. as the criterion of fidelity in performance, so that a maximum of 3·91 bits could be transmitted per sample if the subject were to keep his error to less than $\frac{1}{4}$ cm at each sample-point. For a less stringent criterion, the information per sample would decrease; for example, a unit of 1 cm. ($\pm\frac{1}{2}$ cm.) decreases the input entropy and the maximum rate to 2·91 bits per sample.

The track was found to lag behind the course by a fairly definite amount on any one run (see later discussion and Fig. 4). In principle, this lag represents a distortion which should be corrected by computing the correlation between course and track for various lags, and then adjusting the track by the one giving maximum correlation. (In so far as the lag fluctuates, it represents a calculable loss of information.) However, for each course peak the corresponding track peak was simply identified by visual inspection (see Fig. 4) and for each of the 94 pairs the course and track amplitudes i and o relative to the left-hand edge of the paper were recorded, the amplitude error e being obtained by subtraction. Very occasionally (once in a thousand peaks) it was impossible to find a proper sample point on the track and then the error was measured from the average value of the track.

FIGURE 4



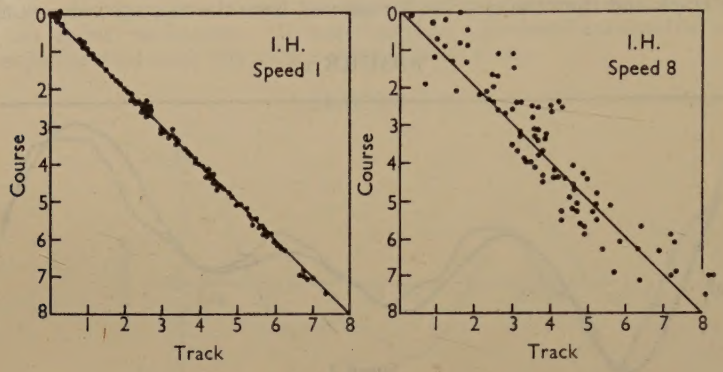
Examples of tracking performance and the identification of amplitude and time errors.

(*a*) Speed 2, no preview. (*b*) Speed 4, no preview. (*c*) Speed 7, no preview.

The 94 pairs obtained from a run were now entered in a scatter-diagram or input-output matrix (Fig. 5). A perfect performance would be represented on the diagram by a diagonal straight line, and complete failure either by a vertical straight line or by a random distribution of dots. The examples given illustrate the fact that performance was found to deteriorate in three ways with increasing speed. First, there was always more scatter

about the mean regression line (more random error or "noise"); second, the mean regression line sometimes had a steeper slope; third, and more rarely, the track was sometimes distributed bimodally, the regression line being then in two parts each parallel to the axis, suggesting that the subject could do no more than get his pointer onto the correct side of the mid-line. The first and third of these represent loss of information, but the second is a distortion, which was automatically allowed for in the later computations.

FIGURE 5



Scattergrams of course and track samples.
(94 samples are plotted; for perfect performance all points would lie on the diagonal).

There are several alternative formulae from which the input, output and joint distributions in the scattergram could be used to compute the entropies and the information-rate (see Shannon and Weaver, 1949, Chap. IV); that of equation (2), was used here. If a very large number of sample-points had been available on each run, the input-output matrix would have yielded functional probability-distributions, and the entropies could then have been evaluated with small error. But there were only 94 samples in each run and since the input sample-points were predetermined, not much improvement could be obtained by repeating runs. With a grouping interval of $\frac{1}{2}$ cm. there were 15 possible sample-values of the course and track and so 15^2 cells in the input-output matrix. If all of them were occupied, the average cell-frequency would be about 0·5; but only 15 to 40 were actually occupied, giving an average frequency of 2 to 5. The estimated joint entropy $H'(i, o)$ was calculated from the cell-frequencies by the formula.

$$H'(i, o) = -\Sigma f(i, o) \log_2 f(i, o) \qquad (4)$$

where $f(i, o)$ = relative frequency of entries in cell (i, o).

(To simplify the computation a table was drawn up to give the contribution of each cell-frequency to the total entropy, and the contributions were then added up directly.) The estimated entropies are perturbed both by sampling error and by systematic bias which are greater for smaller average cell-frequency. Draper (1954) and Hick (1956) have given expressions for them but it was not entirely clear how their formulae should apply in the present case; after trying different formulae the following correction for bias was chosen:—

$$H(i, o) = H'(i, o) - \frac{k - 1}{2N} \qquad (5)$$

where k = number of cells in the occupied region of the matrix
N = number of sample-points.

The correction term never exceeded 10 per cent. and was usually much less. Table I shows the result of applying a different grouping interval to the same data, with and without correction, from which it can be seen that the bias is partly but not completely offset by the correction. As the same interval ($\frac{1}{2}$ cm.) has been used throughout, comparisons between speeds and subjects should, however, be little affected by the residual bias in the present experiments.

TABLE I

THE EFFECT OF GROUPING INTERVAL ON THE ESTIMATED INFORMATION-RATE
(100 sample-points: Subject PW, Speed 4 with preview)

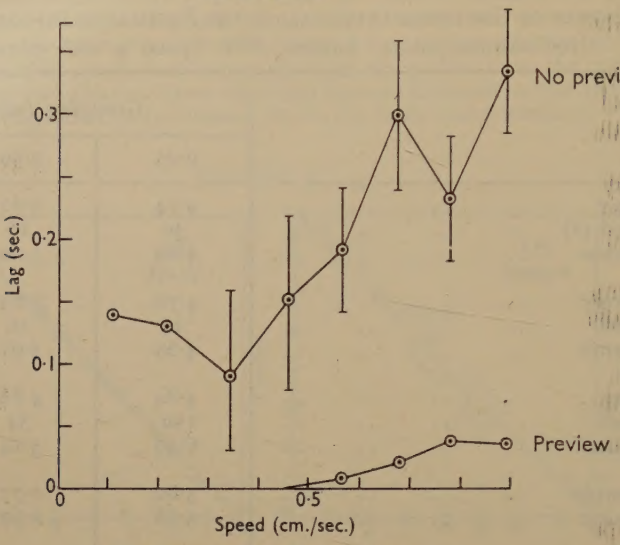
	Grouping Interval (cm.)		
	0·25	0·50	1·00
Input entropy	4·74	3·82	2·88
Cells occupied (k)	30	16	9
Corrected value	4·89	3·90	2·92
Output entropy	4·72	3·83	2·91
Cells occupied	29	16	9
Corrected value	4·86	3·91	2·99
Joint entropy..	4·84	4·88	3·45
Cells occupied	150	54	9
Corrected value	6·59	5·14	3·50
Rate per sample	3·62	2·77	2·35
Corrected value	3·16	2·67	2·41
Rate per sec.	5·25	4·02	3·41
Corrected value	4·58	3·87	3·49

RESULTS

At low speeds (Fig. 4(a)) subjects track accurately. They do not produce smooth tracks with the same bandwidth as the course, as they would were they acting as linear servo-mechanisms. Instead there are many small "ripples" which cross the course several times between sample points, and are presumably due (Hick, 1948) to the discontinuous functioning occasioned by the subject's finite reaction time. The bandwidth of the track being greater than that of the course, the track had to be "smoothed" before computing the rate, and this was done by inspection. At moderate speeds (Fig. 4(b)) the track is very similar to the course in general shape and follows it closely, usually with a small time lag, and some amplitude error; no discrete corrections can now be seen. At high speeds without preview (Fig. 4(c)) the track maintains the general shape and form of the course, but the lag becomes large and substantial errors of amplitude appear. One subject reported "I am doing no good at all; the thing is simply flashing by and I can do no more than make frantic attempts to catch up with it." In spite of this comment, the performance was considerably better than random, to the subject's later surprise. It was clear from the subjects' remarks that at speeds above 2 or 3 without preview, they aim at each successive peak as it comes up, and make no attempt to follow in between. Nevertheless, the track is a good copy of the course, which tends to confirm the rightness of the time-sampling procedure. With preview the deterioration is much less marked. Without preview the time-lag increased with speed up to a certain point (Fig. 6); with it there was a much smaller increase.

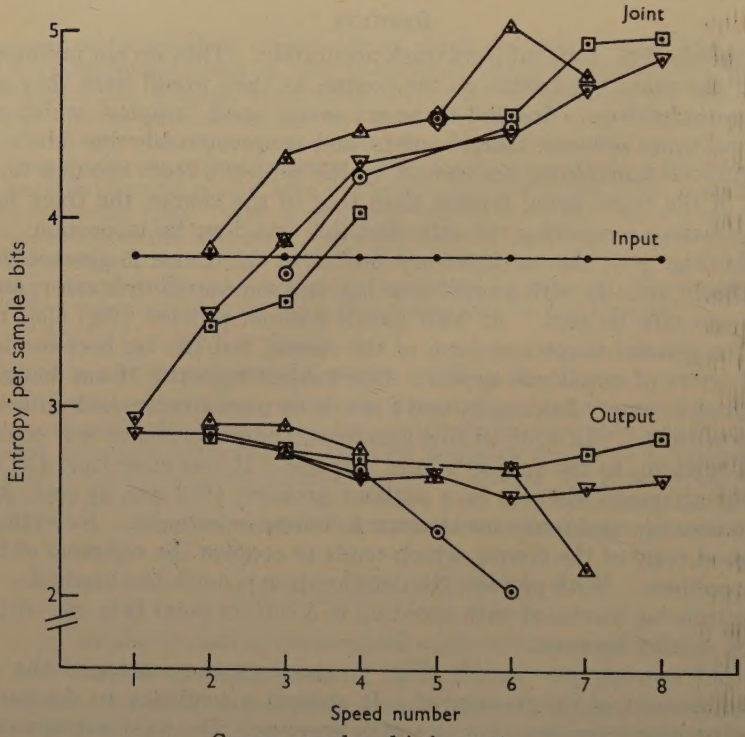
The output entropy per sample (Fig. 7) measures the scatter of the subjects' track or the amount of his movement. It showed a tendency to decrease at the higher speeds without preview, but not with preview. The joint entropy cannot be less than whichever is larger of the input and output entropies, it had this value at low speeds, and rose steadily as the speed increased which indicates a progressive loss of tracking accuracy by the subject, the rise being steeper without preview.

FIGURE 6



The average lag in tracking.
Each point is the average lag over one run for one subject.

FIGURE 7

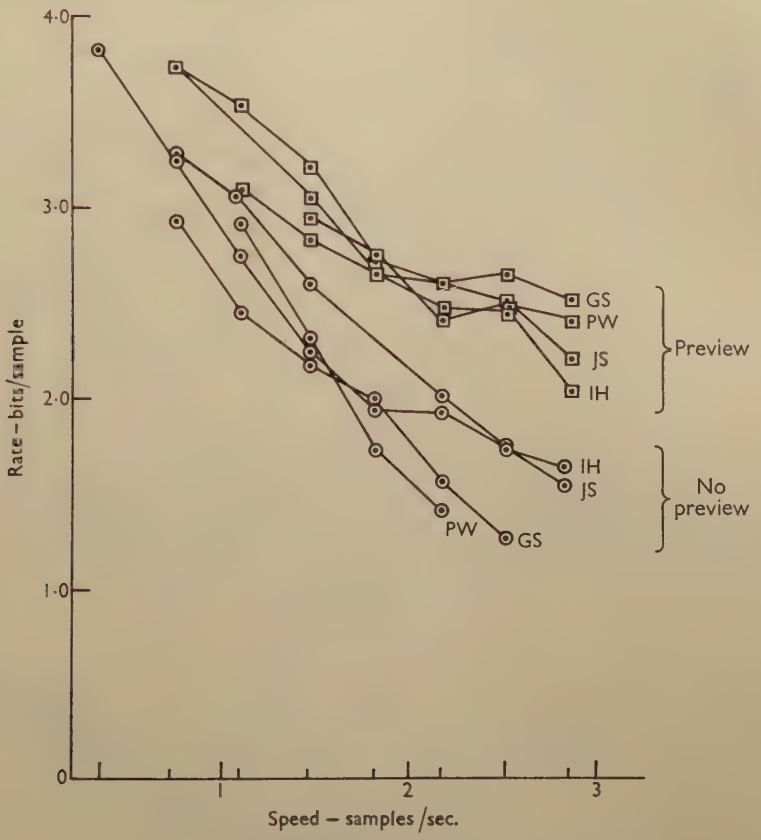


Course-speed and information.

Input, output and joint entropies are shown as a function of course speed. Each point represents one run for one subject. A correction for the sampling bias has been applied.

The information rate per sample (see Fig. 8) started equal to the input entropy, and fell steadily with speed. There was a clear difference between the preview and non-preview conditions at all speeds for all subjects, the preview condition giving the better performance.

FIGURE 8



Course-speed and information.

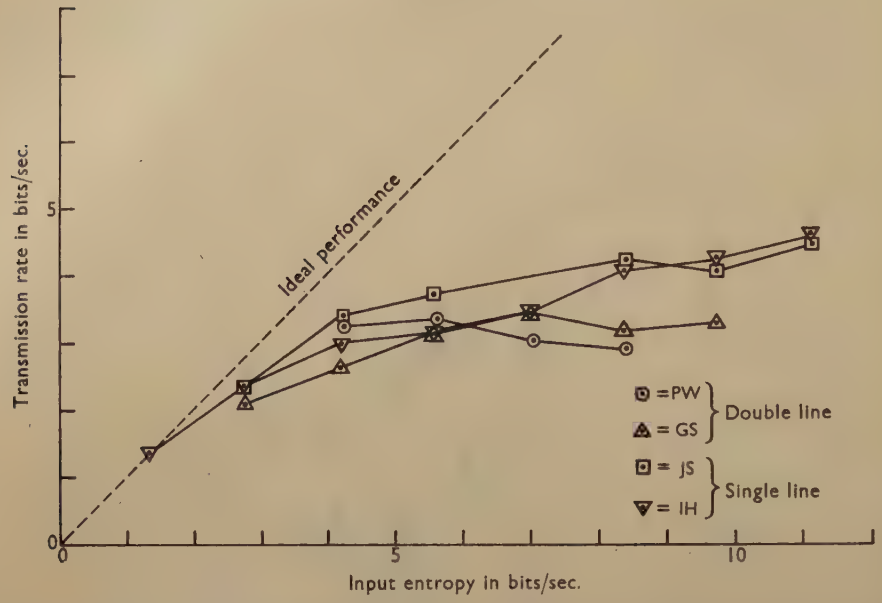
Information rate per sample point is shown as a function of course-speed. Each point represents one run by one subject.

The theoretical maximum information-rate per sec. is equal to the input-entropy, and hence the curve of ideal performance is a line with unit slope. All subjects achieved this at low speeds, beginning to depart from it at about 2 bits per sec. without preview and thereafter performance tended to level off at 3 to 5 bits per sec. (Fig. 9). The flat-topped curve suggests that the subjects had an informational "ceiling" or *channel-capacity* of about 4 bits per sec.

With preview the curve of performance began to depart from the ideal one at about 3 bits per sec. (see Fig. 10), and thereafter increased linearly. The average slope for all four subjects was about half (0·512 times) the ideal slope. At the highest input-entropy possible with the apparatus, there was no sign of the levelling off which would indicate the presence of a definite channel-capacity.

Practice might be expected to have some effect on the rate attainable for any given input speed, and so indeed it did. Table II shows the rates for three successive

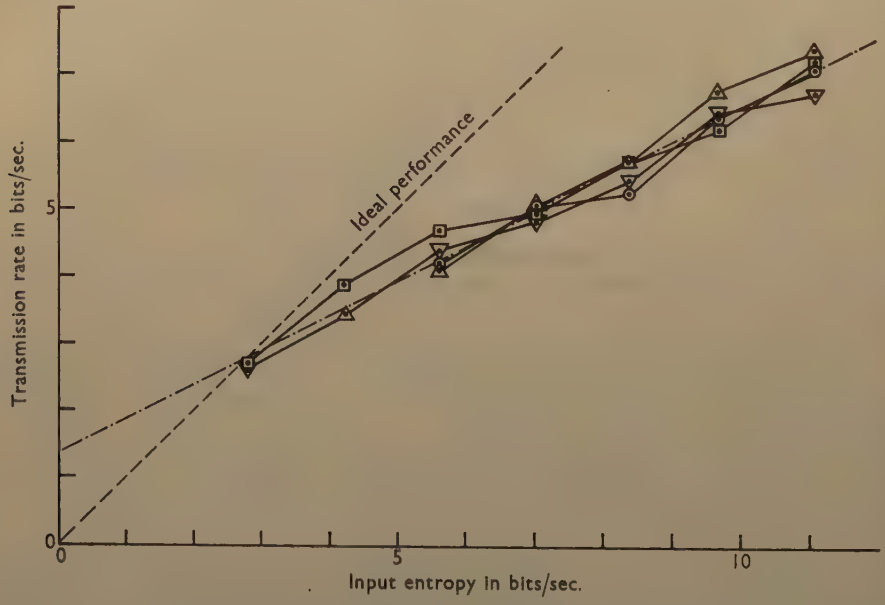
FIGURE 9



Course-speed and information—tracking without preview.

Information-rate per sec. is shown as a function of course-speed for pursuit tracking with minimal preview. Each point represents one run by one subject.

FIGURE 10



Course-speed and information—tracking with preview.

Information-rate per sec. is shown as a function of course-speed for pursuit tracking with ample preview. Each point represents one run by one subject. The dotted line has been fitted by the method of least squares; its equation is

$$R = 0{\cdot}51\ H(i) + 1{\cdot}36 \text{ bits}$$

runs for one subject on the same condition. There was a positive but relatively not large increase in information-rate. This is in line with the results of earlier studies of pursuit tracking. No attempt was made to measure fatigue effects, but within single runs at high speed there were indications from subjects' reports that fatigue was beginning to be felt.

TABLE II

THE EFFECT OF PRACTICE ON INFORMATION-RATE IN PURSUIT TRACKING WITHOUT PREVIEW

(Subject PW; Speed 4)

Run	$H(i)$ *bits*	$H(o)$ *bits*	$H(i, o)$ *bits*	*Rate, bits per sec.*
1.	3·91	3·63	6·20	3·04
2.	3·91	3·74	6·20	3·16
3.	3·91	3·93	6·22	3·32

DISCUSSION

In recent studies Information theory has been used to analyse a number of sensori-motor tasks, and the human channel-capacity has been measured in two distinct ways. In discrete choice-tasks such as light-key reactions (Hick, 1952) and card-sorting when the subject does not know exactly what he will have to do on each trial—his uncertainty being measured by the *input entropy*—the average capacity seems to be about 5 bits per sec., though with very long practice, as in typewriting or piano-playing, (Quastler, 1956) or with high "compatibility" as in target-aiming (Crossman, 1956), he may reach 15 to 20 bits per sec. In these studies information rate may be measured by the total time taken to make a response of given information, or by the increment of time needed to deal with an increment of information, and the two approaches yield rather different results (Crossman and Szafran, 1956). In particular the time taken to complete the response, which is strictly irrelevant since it does not depend on information, has to be small and constant if the two methods of calculating are to give similar results.

The second type of channel-capacity, however, appears in the response itself, as a relation between speed and accuracy. Hand movements only have so far been subjected to study, and the pioneer work is that of Fitts (1954) confirmed by Crossman (1956) and Annett, Golby and Kay (1958). In Fitts' experiment the subject had to tap in turn two metal plates of width w set a distance a apart as fast as he could. The time per tap, t, was found to obey the relation—

$$t = -k \log \left(\frac{w}{a}\right) = -k \, (\log w - \log a) \qquad \ldots \quad \ldots \qquad (6)$$

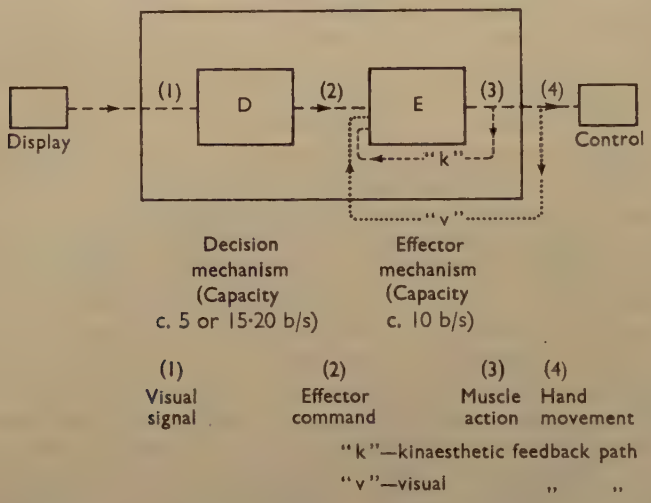
where k is a constant for one subject

a result which will be referred to as Fitts' Law. The right-hand side of the equation can be interpreted informationally (Crossman, 1956) as the difference between an initial entropy log a, and a final one log w, and hence it represents the reduction of uncertainty of the endpoint of movement achieved on a single tap. The information-capacity (the reciprocal of k) was found to lie between 8 and 15 bits per sec. in all the studies mentioned. In this task the subject knows exactly what to do on each trial, so the input entropy is zero. Therefore the time cannot, as in the previous case, be occupied in transmitting the information in the input, and Fitts suggests that it is

taken up in overcoming the "internal random noise" which prevents a subject repeating a movement precisely.

On the evidence of these studies it seems reasonable to postulate that the human sensori-motor apparatus comprises at least two functionally distinct parts (Fig. 11). The first, which may be called the Decision- or D-mechanism, is concerned with translating visual or other signals into orders which the second, the Effector- or E-mechanism, carries out. The D-mechanism has a capacity of 5 bits per sec. in most cases, and up to 15 or 20 bits per sec. in highly practiced or "compatible" tasks; the E-mechanism achieves about 10 bits per sec. for hand-movements. According to this model a subject's performance at a task such as pursuit tracking will depend on exactly how the two mechanisms are loaded and on the time-relations between their activities and the external situation.

FIGURE 11



A schematic representation of information-flow in the human perceptual-motor system.

The Decision (D-) mechanism translates perceived signals into instructions for the effector system.

The Effector (E-) mechanism controls the muscular activity needed to carry out the instructions. It may use visual feedback, "v," and/or kinaesthetic feedback "k."

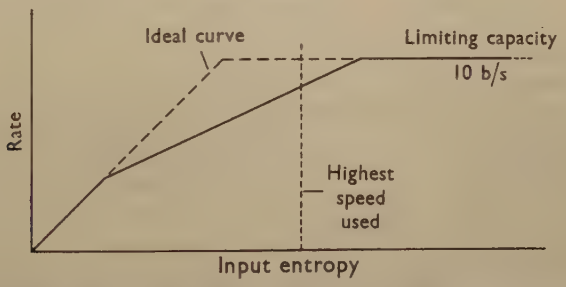
(*a*) *Tracking with preview*

The tracking task certainly had high "compatibility" and the D-mechanism would therefore be expected to work at 15 or 20 bits per sec.: though the control was by hand-wheel rather than direct movement, it seems reasonable to ascribe a capacity of about 10 bits per sec. to the E-mechanism. With preview the subject could look ahead as far as he liked and so offset the delays due to the working of the D- and E-mechanisms, by receptor anticipation (Poulton, 1957). This being so, the smaller of the two capacities should be limiting and one might thus expect an overall capacity of 10 bits per sec. This level was never attained in the experiment (see Fig. 10), though no definite ceiling appeared at a lower level.

The fact that the performance increased linearly with input load, yet at less than the ideal slope, suggests that another limitation, one allowing a constant fraction, rather than a constant absolute amount of information to be transmitted, must have been

present, and its locus may be sought in the division of visual attention between course and error. At high speeds, when looking ahead at the course, the subject was only able to see his own pen in peripheral vision, and actually to look at it would have entailed missing some of the course. Now the information-capacity measured by Fitts was for a fully visual task, represented in Figure 11 by the E-mechanism with the visual feedback path "v" in action. An equivalent task done without visual control has been studied by Vince (1948) who reports that hand movements of a few inches extent made without vision were distributed normally with about $\pm$ 7 per cent. error, and took 0·25 sec. or less. This much error is equivalent to $\log_2 100/14$ or about 2·8 bits per movement. In the present task, it would imply that as soon as the subject's visual channel, attending to the course some distance ahead of his pen, were loaded sufficiently near to its full capacity of 15 or 20 bits per sec. to be confined there, the information transmitted per *sample* should settle at about 2·8 bits, and the rate per sec. would be given by 2·8 $\times$ (number of samples per sec.), until the E-mechanism's upper limit of 10 bits per sec. were reached. The observed performance curve agrees reasonably well with this view, and it may be noted in its support that the curve of transmitted information (Fig. 8) does appear to flatten out at about 2·5 bits per sample. Figure 12 shows how the performance curve for the preview condition might be expected to behave at loads greater than those used in the present experiment. It is also noteworthy that subjects reported that they had adequate time for movement at even the highest speeds in the preview condition. If this explanation is correct, then at high speeds with preview subjects made purely positional responses to successive peaks and ignored the velocity and acceleration of the track in between.

FIGURE 12



The expected behaviour of subjects tracking at higher speeds than those used. The upper limit represents a channel-capacity of 10 bits per sec.

(*b*) *Tracking without preview*

Without preview the subject's attention must be centred on the narrow slit in which both course and controlled member appear. Bearing in mind that subjects were instructed to *reproduce* rather than strictly *track* the course, and that lag did not necessarily diminish their computed performance, the capacity expected according to the model would be just the same as in the preview condition, but in fact a capacity of only 4 bits per sec. was found. Again according to the model, the lag should be $\left(\frac{3{\cdot}91}{20} + \frac{3{\cdot}91}{10}\right)$ or about 0·6 sec., if the subject makes a positional response to each peak in turn; yet the observed lag (Fig. 6, Table III) was less than half this amount, which suggests that subjects may have been sacrificing information (i.e. accuracy of reproduction) to reduce their lag. Least lag is introduced by responding quickest, that

is at low accuracy, but for correct tracking each response must on average contain as much information as the track does in the same time. Therefore if the subject is trying to reduce his lag at low track speeds, one would expect to find him making frequent responses of low accuracy, and vice versa. In order to examine this point, one subject's records were examined closely; it was found that individual response movements could be distinguished sufficiently well to count them and measure their duration, with results given in Table III. At low speeds (1, 2 and 3) the expected behaviour clearly occurs; it also appears that even when allowance is made for the residual receptor anticipation permitted by the small preview, this subject must have been predicting course positions (perceptual anticipation) to some extent, since response movements were consistently initiated before their targets came into view. At higher speeds (4 and above) the number of movements fell to one per sample-point as had been expected, but the lag was still less than the movement-time at all speeds except possibly the highest, and the subject must therefore always have been anticipating course positions.

TABLE III

	Speed							
	1	2	3	4	5	6	7	8
(*a*) *Characteristics of the tracking task*								
Paper speed (cm. per sec.)	1·09	2·22	3·40	4·53	5·62	6·75	7·80	8.93
Time per sample (sec.)	2·78	1·37	0·89	0·67	0·54	0·45	0·39	0·34
Input entropy (bits per sec.)	1·41	2·86	4·38	5·84	7·24	8·69	10·00	11·50
Preview time (8 cm.) (sec.)	7·3	3·6	2·4	1·8	1·4	1·2	1·0	0·9
,, ,, (½ cm.) (sec.)	0·46	0·23	0·15	0·11	0·09	0·07	0·06	0·06
(*b*) *Information Rates* (average of four subjects)								
Per sample—with preview (bits)	3·86	3·66	3·26	2·92	2·67	2·48	2·50	2·40
,, without ,, ,,	3·86	3·18	2·83	2·35	1·91	1·66	1·61	1·77
Per sec.—with preview ,,	1·39	2·67	3·66	4·35	4·95	5·50	6·42	7·07
,, without ,, ,,	1·39	2·32	3·18	3·50	3·54	3·68	4·10	5·20
(*c*) *Tracking without preview: an analysis of one subject's performance*								
Average lag (sec.)	0·14	0·13	0·09	0·15	0·19	0·30	0·23	0·33
s.d. of lag (sec.)	—	—	0·07	0·07	0·05	0·06	0·05	0·05
Average duration of response movements (sec.)	0·45	0·32	0·30	0·33	0·45	0·44	0·38	0·33
No. of movements per sample-point	4–8	3–4	2–3	1–2	1–2	1	1	1
Information per movement (bits) (for perfect performance)	½–1	1–1·3	1·3–2	2·0 or 3·9	2·0 or 3·9	3·9	3·9	3·9

According to Poulton (1952, 1957) and others, prediction of the future position of a course is based mainly on its speed at the time of observation. If the subject were capable of making such a prediction with the same accuracy and speed as he can observe position itself, the changeover would entail no loss in information-rate, but this seems unlikely as the accuracy of prediction must inevitably diminish with its range, and prediction is also perceptually more complicated than direct observation of position. In terms of the model, then, one might suppose the prediction to be carried out by a third mechanism with a capacity of 4 bits per sec. approximately, whose

output would pass to the D-mechanism for translation into motor instruction: but a more parsimonious explanation is suggested by the similarity between the rate found here and that observed in choice-tasks of low compatibility (e.g. Hick 1952), which leads one to suppose that the prediction task is an "incompatible" one and that the capacity of 4 bits per sec. can simply be ascribed to the D-mechanism operating in its usual manner. This hypothesis could be tested by setting up a simple choice-task involving prediction and measuring the capacity directly.

If it is indeed the use of prediction which lowers the rate, a change might be expected to occur when the course speed no longer allows time for it, that is, when peaks recur at less than about 0·3 sec. The subject should then be forced into making pure positional responses lagging by just the spacing of one sample, and the rate should rise steadily with speed from 4 to 10 bits per sec. Unfortunately the highest track speed available fell just short of the point where this would begin to happen.

It remains to consider why the subject should avoid lagging by the time (about 0·6 sec.) needed for pure positional responses. Four reasons can be suggested: (1) that subjects could have done so but were given insufficient practice to overcome their initial reluctance to "get behindhand"; (2) that too great difference between the current motions of course and track would have produced intolerable visual confusion; (3) that course information might have been forgotten during such a comparatively long delay; (4) that too little feedback information about the response movements would then have been available. Further work will be needed to decide between these alternatives.

Conclusions

The informational analysis of pursuit-tracking performance appears to be feasible and to yield fresh understanding of the mechanisms involved. According to the relatively crude results of the present study the subject's performance seems capable of being fully explained in terms of a theoretical model derived from the results of previous work. In particular, the capacity in tracking with preview is just that of the effector system acting without visual feedback at about 2·5 bits per response or 10 bits per second, whichever is less. Without preview and when the lag is kept small the limit appears to be set at about 4 bits per sec. by the decision-mechanism predicting future course-positions.

It is hoped to carry out further work on these lines, with certain improvements. First, the present study was hindered by the need to use a predetermined course pattern, which had too much autocorrelation to be really satisfactory. It now appears technically feasible to provide a truly random course-generator of limited bandwidth. Second, it is possible that the speed of eye-movements may limit certain features of performance, such as the division of attention between course and track in the preview condition; eye-movements should therefore be recorded and compared with the current hand movements. Third, in the discussion free use has been made of information-capacities drawn from earlier work. Since there is considerable variation between individuals, a much more accurate analysis would be possible if each subject's capacity were measured on subsidiary tasks at the time of his tracking performance. Fourth, the present approach could also be extended to study the effect of statistical structure in the course, that is, how far the subject can make use of observed patterns of course behaviour to facilitate his performance.

Finally it may be said that the statistical information-theory approach to the study of tracking outlined in this paper is quite compatible with the analytic approach. It does, however, appear that the informational conditions of tracking should be

closely specified before a definite transfer-function can be identified by analytic means, and that its parameters may be expected to change with the statistical structure of the task.

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CONCERNING THE INTERRELATION BETWEEN ABSOLUTE SENSITIVITY AND STRENGTH OF THE NERVOUS SYSTEM

BY

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SUMMARY*

This paper is concerned with the relation between absolute sensitivity and the "strength" of the C.N.S., in the sense in which the term was used by Pavlov. The authors advance the hypothesis that "weakness" of the nervous system is correlated with high reactivity, and hence with high sensitivity to peripheral stimulation; conversely, "strength" of the nervous system is correlated with low reactivity, and hence with low sensitivity to peripheral stimulation.

Two sets of experiments were carried out in order to test this hypothesis, one concerned with visual and the other with auditory sensitivity. In the visual experiments, absolute sensitivity to light in the periphery of the dark adapted eye was measured on 26 subjects by means of an adaptometer. Measurements were taken at two minute intervals over a period of about half an hour, the mean threshold value for each subject being calculated from the results obtained at a number of experimental sessions distributed over 3 to 5 days. In the auditory experiments, absolute threshold for sound intensity was determined in 22 subjects. A pure tone of 1,000 c.p.s. was fed to the subject binaurally through headphones and the sound pressure level systematically varied by the experimenter. Threshold measurements were made at 1 min. intervals over a 15 to 20 min. period. Sensitivity is calculated as a value inversely proportional to that of the threshold.

In order to determine the "strength" of nerve cells, the authors made use of a method involving a conditioned decrease of sensitivity to light or sound. The rationale of the method is as follows: The strength of a conditioned reflex is normally found to increase with increase in intensity (or duration) of the conditioned stimulus until a "physiological limit" is reached. Above this limit, further increase in stimulus intensity no longer produces an increase in response, which may indeed diminish as a consequence of "transmarginal inhibition." Hence a determination of the stimulus intensity above which there is no increase in response strength provides a method of defining the physiological limit, or "strength," of the central nervous processes involved.

The actual method used here consisted in exposure for 10 to 15 sec. of an illuminated panel as the unconditioned stimulus, the resulting reduction in peripheral visual sensitivity being the unconditioned response. A pure tone of 1,000 c.p.s. and a red light of low intensity were used as conditioned stimuli. The conditioned stimulus was applied 5 sec. before the onset of the unconditioned stimulus, the duration of their combined application being 10 sec. After the conditioned reflex was well established the tests of "strength" were begun. These consisted in the application of the conditioned stimulus (always reinforced) ten times in succession at intervals of 2 min. The conditioned stimulus was also presented twice without reinforcement, once at the beginning and again at the end of the series. If the strength of the conditioned reflex showed no decrement on this second trial as compared with the first, it was concluded that the physiological limit or "strength" of the corresponding central nervous structures was high; if, however, there was decrement, this was taken to mean a low physiological limit or "weakness." The criterion of decrement was a decrease in strength of the conditioned reflex of 10 per cent. or more.

These experiments were also repeated after the administration of 0·2 gr. of caffeine, which is thought to raise the excitability, and hence diminish the "strength," of central nervous processes.

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The results appear to demonstrate a negative correlation between sensitivity, visual or auditory, and the "strength" of nervous processes. In general, subjects with "weak" nervous systems gave low absolute threshold values, indicating high sensitivity, whereas those with "strong" nervous systems gave high threshold values, indicating low sensitivity. There were, however, some exceptions to this rule.

In discussion of these results, the authors lay stress on functional changes which are believed to occur in neurones as a consequence of sustained excitation. In general, they argue that both sensitivity and "strength" of nervous processes can be related to a more basic dimension of neural "reactivity."

I. Introduction

Pavlov and his collaborators showed that the strength of the nervous system is one of the most important properties which determines individual differences of the higher nervous activity in animals and man. The parameter of the strength of the nervous system was taken by Pavlov as a basis for his classification of the types of nervous system.

According to Pavlov, the strength of the nervous system is characterized mainly by the limit of the working capacity of the nerve cells, i.e. by their capacity to endure a protracted and concentrated excitation or action of a very strong stimulus, without passing into the state of inhibition. As to the weakness of the nervous system, this is expressed in a low working capacity or a low limit of endurance of the nerve cells. Externally this is manifested in the emergence of transmarginal inhibition when the intensity or duration of the action of the stimulus is increased beyond a definite limit.

Thus, from the point of view of the endurance of the cells the weakness of the nervous system is a negative property. Taking only this aspect into consideration one may, naturally, regard the weak type of nervous system as a "bad" one, and the strong type as a "good" one. Such a viewpoint appears almost self-evident. Pavlov himself adhered to it, characterizing the weak type of nervous system as a "more or less invalid" type.

This viewpoint, however, seems to us one-sided and consequently erroneous.

As shown by the analysis of life experience, persons with a weak nervous system, i.e. a nervous system characterized by low endurance, easy fatiguability, etc., exhibit a number of peculiar features showing that their nervous system possesses not only negative but also manifestly positive properties.

This is confirmed by some of the theoretical propositions of Pavlov himself. In his works written in the period from 1922 to 1927 he repeatedly expressed the idea that the rapid exhaustibility of weak nerve cells is closely connected with their "extreme irritability," or "easy excitability" (Pavlov, 1926).

These facts and theoretical propositions, which we expounded in greater detail elsewhere (Teplov, 1956), have led us to the hypothesis that *the weakness of the nervous system is internally connected with its high reactivity, or high sensitivity.*

The present work is devoted to the experimental corroboration of this hypothesis.

II. Methods of Investigation

The principal method used by us was the method of comparing experimental data obtained from one and the same person and relating, on the one hand, to absolute sensitivity, and, on the other hand, to the strength of the nervous system.

1. *Determination of sensitivity*

(*a*) *Visual sensitivity.* The measurements of the sensitivity of peripheral vision are carried out with the help of an adaptometer. The subject is placed in a dark room. The white circle of the adaptometer (having a diameter of 1°30′) is fixed on the wall in front of the subject. At the side of the circle (angular distance—10°) is a red fixation dot which serves for fixing the eyes of the subject during the measurement of sensitivity. A ray of light from the adaptometer (placed behind the subject) falls upon the circle. The

ray is intersected by an absorbing photo-filter. Moving the latter from one place to another, the experimenter is able gradually to change the degree of illumination of the circle. The experiment is preceded by a dark-adaptation during 45 min. At the signal "attention!" the subject fixes his eyes on the red dot. He has to ascertain, as precisely as possible, the moments of emergence and disappearance of a just perceptible bright spot in the place of the circle and immediately to inform the experimenter. The latter fixes the corresponding divisions on the photo-filter which indicate the percentage of the absorption of light. The mean of these two values characterizes the threshold of visual sensation. The sensitivity is calculated as a value which is inversely proportional to that of the threshold. Measurements are taken at intervals of 2 min. during a period from 20 to 40 min., until the indications of the subject acquire a uniform and stable character. The average value is deduced on the basis of the results obtained during 3 to 5 experimental days.

(*b*) *Auditory sensitivity.* The measurement of auditory sensitivity is carried out with the help of a sound generator in a room relatively insulated from outside noise. The sound (a pure tone of 1,000 c.p.s.) is conveyed to the subject binaurally through ear-phones. Smoothly turning the handle of the rheostat, the experimenter gradually modifies the output voltage and, consequently, the level of sound pressure in the acoustic duct of the subject, first increasing it and then decreasing. The subject must ascertain the moments of emergence and disappearance of the sound and immediately inform the experimenter. The latter fixes corresponding divisions on the scale which is graduated in units of sound pressure. The mean of these two values characterizes the threshold of auditory sensation. The sensitivity is calculated as a value which is inversely proportional to that of the threshold. Measurements are taken at intervals of 1 min. during a period from 15 to 20 min., until the indications of the subject reach stability.

2. *Determination of the strength of nerve cells*

A special method of determining the strength of nerve cells has been elaborated. It was necessary to establish the individual limits to which concentrated excitation in the nerve cells can be increased. According to the so-called "law of force," the strength of a conditioned reflex increases with the growing intensity of the conditioned stimulus, all other things being equal. If, however, the intensity of the stimulus oversteps the limits of the working capacity of the nerve cells, the strength of the conditioned reflex no longer increases; on the contrary, it diminishes, since "transmarginal" or "protective" inhibition comes into force. A similar effect is obtained if we increase the duration of the stimulus and not its intensity; in this case the stimulus may act not continuously, but with interruptions. It is only important that this stimulus should evoke well-concentrated excitation in the cerebral cortex; excitation in the focus of the conditioned stimulus as a result of a firmly elaborated conditioned reflex fully complies with this requirement.

In our experiments, we used a conditioned decrease of sensitivity to light or sound. The unconditioned stimulus consisted in a momentary (10 to 15 sec.) illumination of a panel placed before the subject and occupying his entire field of vision. The intensity of illumination at the subject's eye-level was about 40 lux. As a result of this stimulus, sensitivity of peripheral vision was as a rule reduced by some 50 to 80 per cent.; in 2 to 5 min., its sensitivity was restored to its previous level.* A pure tone of 1,000 c.p.s. conveyed to the subject from a sound generator through ear-phones and a weak optic stimulus of red colour (which does not disadapt the rods of the retina) were used as conditioned stimuli. In some cases the very first presentations of these stimuli produced a small increase or decline of sensitivity (by 10 to 20 per cent). These changes in sensitivity were, apparently, manifestations of a general orienting reaction, since they were readily extinguished in the course of one or two experiments. After the extinction of the orienting reactions we combined the conditioned stimulus which acted during 15 sec. with a 10-sec. bright illumination, the conditioned stimulus being put into action 5 sec. earlier than the unconditioned stimulus. After a series of such combinations a conditioned connection was established between the action of the conditioned stimulus and the reaction of a decline of visual sensitivity. This was shown by the fact that the conditioned stimulus, when applied alone, began to cause a similar decline of sensitivity. This

* The rôle of the pupil changes in this conditioned reflex has been specially investigated by V. I. Rozhdestvenskaya (1955), who was able to show that a conditioned decrease of sensitivity to light is also observed under conditions in which pupil size is maintained constant by means of homatropine. Under such conditions, however, the strength of the reflex declines considerably.

decline in general amounted to 15 to 30 per cent. of the background level of sensitivity, i.e. it does not reach the strength of the unconditioned reflex, which usually lies within the range of 50 to 80 per cent. The conditioned decline can still be observed 30 to 60 sec. after the termination of the conditioned stimulus.

After the elaboration of a well-established conditioned reflex (which could be seen from the occurrence of this reflex in not less than three successive experiments) we performed our first control experiment of testing the "strength" of the nervous system. In this experiment the conditioned stimulus, now calling forth a concentrated excitation (every time accompanied by a reinforcement in order to prevent the extinction of the conditioned connection), was presented ten times in succession at intervals of 2 min. Thus, concentrated excitation was produced in the same cells repeatedly. We assessed the state of the cells by the strength of the conditioned reflex. For this purpose, the conditioned stimulus was twice presented in isolation, i.e. without reinforcement: the first time at the very beginning of the experiment, and the second time immediately after its tenth presentation in combination with a reinforcement. If the strength of the conditioned reflex in the second test remained on the same level as in the first test, this was taken to indicate the high working capacity, or "strength" of the nerve cells. On the other hand, a decline of the strength of the reflex was taken to indicate a low working capacity, or "weakness" of the nerve cells. The cause of this decline lies in transmarginal inhibition arising in the cells whose low working capacity prevents the further growth of excitation within them.

The second control experiment on testing the "strength" of the nervous system was performed after the administration of 0·2 gr. of pure caffeine. Caffeine raises the excitability of the nerve cells, as a result of which the limit of their working capacity is reached at a smaller intensity or duration of the action of the stimulus. As was to be expected, after the administration of caffeine the decline in the strength of the conditioned reflex, as a result of its frequent repetition in combination with a reinforcement, was of a more pronounced character. In a number of cases the strength of the reflex remained unchanged in experiments not accompanied by the administration of caffeine, while in experiments where caffeine was applied we noted a decline. In these cases we have, apparently, an intermediate level of strength of the nervous system.

Using the terminology accepted in Pavlov's laboratories, we can define the aforementioned methods as *methods of extinction with the reinforcement of a "photochemical" conditioned reflex.*

In an earlier study (Nebylitsyn, 1957) we established that tests of the strength of the nervous system by the method described indicate primarily the strength of the nerve cells in the region of the brain to which the conditioned stimulus is addressed. Indeed in some subjects, extinction trials accompanied by reinforcement yielded different results according to the modality of the conditioned stimulus. We therefore endeavoured to determine the strength of the cells in the visual cortex by experiments using a conditioned visual stimulus, and the strength of the cells in the auditory cortex by experiments using a conditioned acoustic stimulus.

3. *Subjects*

The subjects were mainly students, of both sexes, with an age-range from 18 to 30. Four older subjects, with ages ranging from 43 to 55, were also employed. Before the extinction trials all subjects were thoroughly trained in making observations, the accuracy of which were specially checked by the experimenter.

III. Results

A. Determination of visual sensitivity and of the strength of the cells of the visual cortex

The results of this series of experiments are shown in Table I. It will be seen that the range of individual differences in sensitivity is very considerable. Thus the sensitivity of subject No. 1 in Table I is almost tenfold that of subject No. 26. These variations are in keeping with the data obtained by other authors. It will also be noted that, within the group as a whole, the numerical values of sensitivity form a continuous series.

When determining the strength of nerve cells, the criterion employed is a decrease in the strength of the conditioned reflex by 10 per cent. or more. This decrease is measured, not from the initial strength of the conditioned reflex, but from the

"background level" of sensitivity measured after the extinction trial accompanied by reinforcement. It is plain, however, that we cannot by such a method obtain gradations in the strength of nerve cells comparable to those of sensitivity. Our present methods allow us only to divide the subjects into two groups, those with weak and those with strong nerve cells respectively.

TABLE I

ABSOLUTE VISUAL SENSITIVITY IN COMPARISON WITH THE RESULTS OF EXTINCTION TRIALS ACCOMPANIED BY REINFORCEMENT

Subjects	*Absolute sensitivity (in conventional units)*	*Strength of conditioned reaction in percentages of decline (or increase) of sensitivity*				*Strength of nerve cells*
		Without caffeine		*Caffeine 0·2*		
		Before extinction	*After extinction*	*Before extinction*	*After extinction*	
1	24·5	−26	−16	−26	+ 6	weak
2	23·3	−27	0	−30	+12	weak
3	22·1	−40	−18	−22	+ 5	weak
4	19·6	−32	−13	−24	0	weak
5	18·4	−45	−53	−29	−39	strong
6	18·4	−26	− 5	−15	− 7	weak
7	17·2	−28	−15	−40	0	weak
8	15·9	−21	+ 5	−27	+35	weak
9	15·9	−27	+14	−18	+18	weak
10	15·0	−38	− 6	−15	+ 9	weak
11	15·0	−30	−11	−30	0	weak
12	13·4	−25	− 5	−40	− 5	weak
13	12·7	−19	−46	−38	−22	intermediate
14	10·2	−20	−13	−26	−18	strong
15	8·9	−26	−23	−24	−40	strong
16	8·9	−23	−49	−27	−46	strong
17	8·9	−15	−49	−17	−38	strong
18	8·5	−38	−38	−39	−38	strong
19	8·1	−20	−32	−26	−28	strong
20	6·7	−20	−35	−30	−37	strong
21	6·7	−21	−42	−29	−41	strong
22	6·3	−30	−13	−26	− 6	weak
23	6·0	−27	−21	−20	−17	strong
24	6·0	−23	−36	−27	−60	strong
25	5·6	−20	−25	−26	−20	strong
26	2·5	−27	−38	−22	−15	strong

The first group comprises 12 subjects (cf. Table I). In two of these (Nos. 8 and 9), the conditioned stimulus, after the extinction trial accompanied by reinforcement, not only failed to evoke the usual decline but actually led to an increase in sensitivity. This increase is still more pronounced in experiments with administration of caffeine. The experiments of Demirchoglyan and Allakverdyan (1957) have already shown that inhibition, which develops when a conditioned "photochemical" reflex is extinguished without reinforcement, is accompanied by an increase in sensitivity. Similar reactions of an opposite sign have been recorded by Ilyina (1958) in experiments involving the application of differential stimuli. These findings give us grounds to believe that an increase in sensitivity after an extinction trial with reinforcement (as in the present experiments) is due to transmarginal inhibition of a more profound

character than in the case of a simple decline in the strength of the reaction. It must be assumed that individuals showing this phenomenon possess the weakest nerve cells.

The second group comprises 13 subjects (cf. Table I). In these individuals the strength of the conditioned reflex does not decline after extinction trials accompanied by reinforcement—indeed in some cases (Nos. 15, 16, 17, 21 and 24) it actually increases. This testifies to the fact that a further intensification of excitation in the relevant cortical cells is still possible; that is to say, the limit of their working capacity has not yet been reached. These subjects may be regarded as having the strongest nerve cells in the visual cortex.

The results in one subject (Table I, No. 13) were somewhat anomalous. In this case, although the strength of the conditioned reflex after an extinction trial under ordinary conditions did not increase, it declined considerably following the administration of caffeine. This suggests that the strength of the relevant nerve cells in this subject is at an intermediate level.

Let us now consider the distribution of these data with regard to the measurements of absolute sensitivity. It will be seen from Table I, in which subjects are enumerated in order of diminishing sensitivity, that 11 of the 12 subjects with weak nerve cells occupy the top 12 places in the table while 11 out of the 13 subjects with strong nerve cells occupy the bottom 12 places in the table. It would therefore appear that the subjects with the highest visual sensitivity are almost without exception those possessing weak cells in the visual cortex: and conversely, that subjects showing a relatively lower absolute visual sensitivity level are in the overwhelming majority of cases those possessing strong cells in the visual cortex.

These results give us ground to state that, in general, there is concurrence between high visual sensitivity and a weak visual cortex and between low visual sensitivity and a strong visual cortex. There are, however, two exceptions to this rule (Table I, Nos. 5 and 22). Further, we did not observe any gradual interdependence between sensitivity and strength within each of the basic groups. These two subjects (Nos. 8 and 9), who according to our criteria possessed the weakest nerve cells, did not reveal the highest sensitivity, and five subjects (Nos. 15, 16, 17, 21 and 24) regarded by us as possessing the strongest nerve cells by no means exhibited the lowest sensitivity. Further, the two subjects (Nos. 25 and 26) who stand at the bottom of Table I did not exhibit any signs of maximal strength. Indeed, in the caffeine experiments these subjects not only fail to manifest any increase in the strength of the reflex but even show a slight decline. In view of these considerations, it follows that the relation between sensitivity and strength is not of a functional nature but must be regarded simply as a correlation apparent when the group is considered as a whole.

A statistical analysis of the data by means of the Fisher z-test (Snedecor F test) revealed that the difference between the arithmetical means of the visual sensitivity thresholds in the two groups was highly significant ($F = 27{\cdot}59$; $p < 0{\cdot}001$, the degrees of freedom being 1 and 23).

The results of this series of experiments fully confirm our hypothesis concerning the co-ordination between sensitivity and strength of the nerve cells. High visual sensitivity is combined with weakness, and low sensitivity with strength, of nerve cells in the corresponding areas of the brain.

B. Determination of auditory sensitivity and of the strength of the cells of the auditory cortex

The results of this series of experiments are presented in Table II. As with the visual experiments, we note a wide range of individual differences in auditory sensitivity—the value given by subject No. 1 in Table II being over five times greater than that given by No. 22.

TABLE II

ABSOLUTE AUDITORY SENSITIVITY IN COMPARISON WITH THE RESULTS OF EXTINCTION TRIALS ACCOMPANIED BY REINFORCEMENT

Subjects	*Absolute sensitivity (in conventional units)*	*Strength of conditioned reaction in percentages of decline (or increase) of sensitivity*				*Strength of nerve cells*
		Without caffeine		*Caffeine 0·2*		
		Before extinction	*After extinction*	*Before extinction*	*After extinction*	
1	27·0	−20	+14	−25	0	weak
2	26·0	−39	+ 7	−41	+ 3	weak
3	21·1	−22	+ 6	−25	0	weak
4	20·1	−28	0	−35	− 5	weak
5	18·0	−28	−10	−29	−10	weak
6	18·0	−25	−22	−18	0	intermediate
7	14·2	−28	−39	−28	−13	intermediate
8	13·2	−28	−38	−17	−34	strong
9	12·9	−32	0	−29	0	weak
10	12·1	−36	−24	−23	− 5	intermediate
11	11·7	−23	−41	−26	−34	strong
12	11·7	−40	−12	−23	− 7	weak
13	10·7	−29	+10	−34	0	weak
14	9·7	−17	−16	−30	− 6	intermediate
15	9·6	−22	−33	−22	−15	strong
16	9·1	−21	−26	−19	−46	strong
17	8·2	−28	−26	−23	−30	strong
18	7·8	−23	−21	−22	−31	strong
19	6·3	−28	−50	−20	−32	strong
20	6·2	−24	−19	−26	−20	strong
21	6·2	−23	−34	−16	−10	strong
22	5·0	−26	−32	−23	−17	strong

These data permit us to classify the subjects into three groups according to relative cerebral strength, viz. those with weak nerve cells (Group 1), those with strong nerve cells (Group 2), and an intermediate group (Group 3) (cf. Table II).

Group 1 comprises eight subjects, in all of which a decline in strength of the conditioned reflex following an extinction trial accompanied by reinforcement was observed. Group 2 comprises 10 subjects and in these, the strength of the conditioned reflex did not change following an extinction trial accompanied by reinforcement. Group 3 comprises four subjects in which the results were intermediate between those of Groups 1 and 2. In these four subjects, the strength of the conditioned reflex declined only after the administration of caffeine.

As regards the distribution of the subjects in these three groups with regard to auditory sensitivity level, the picture is somewhat more complicated than in the case of the visual experiments. It is true that the top five places in Table II are occupied by members of Group 1, but the other three subjects in this group occupy the 9th, 12th and 13th places respectively. Thus five subjects with a weak auditory cortex possess high sensitivity and the remaining three medium sensitivity. In this same way, the bottom eight places in Table II are occupied by members of Group 2, whereas

the remaining two subjects in this group occupy the 8th and 11th places respectively. Thus, while the majority of subjects with strong nerve cells possess relatively low auditory sensitivity, two show medium sensitivity. All four subjects in Group 3 will be seen to possess medium sensitivity.

Taking the picture as a whole, it will be seen that whereas the subjects at the two ends of Table II (viz. Nos. 1 to 5 and 15 to 22) show uniform level of strength, there is a zone (viz. Nos. 6 to 14) which includes all three levels of strength as revealed by our method of assessment. Thus we can state that the conformity between absolute sensitivity and strength of the nerve cells does not clearly hold in the case of the intermediate threshold values. But this conformity increases steadily as we approach either the higher or the lower threshold values for absolute sensitivity.

Statistical analysis revealed that the difference between the mean auditory sensitivity values in Groups 1 and 2 was highly significant ($F = 21{\cdot}42$. $p<0{\cdot}001$; the degrees of freedom being 1 and 16).

The results of these experiments provide confirmation of our hypothesis regarding the correlation between sensitivity and strength of the nervous system—weak nerve cells corresponding to higher sensitivity and strong nerve cells to lower sensitivity.

IV. Discussion

The data obtained in these experimental investigations of two sensory modalities have confirmed the hypothesis of the existence of a negative correlation between sensitivity and strength of the nervous system. We have now to elucidate the nature of the relationship between these two seemingly different parameters of nervous activity.

The concept of strength of the nervous system can be defined in terms of the functional stability of nervous tissue when subjected to conditions of stimulation which, from the standpoint of intensity or duration—or both—approach or even exceed the normal physiological limit. When, therefore, we speak of the strength of the nervous system as a typological property we have in mind the limit of working capacity as determined by the threshold of functional endurance: the higher this threshold, the greater the strength of the system.

We shall approach the concept of sensitivity from the standpoint of the internal physiological conditions which determine the excitability of the central nervous mechanisms governing sensory response. The term "threshold of excitability" (and in consequence "threshold of sensitivity") usually implies that the energy of stimulation, upon reaching a definite threshold value, evokes a state of excitation in the receptor mechanism. Hence thresholds of sensitivity, as psychophysically conceived, are primarily dependant upon excitability thresholds in the receptor and central mechanisms. The question of the nature of sensitivity thus resolves itself into a question of the nature of the basic excitatory process.

In 1940, Nasonov developed a theory of excitation based upon the concept of "paranecrosis" which has not become generally known outside its country of origin. Nasonov and his collaborators claim that cells react to damage with a complex of structural changes ("paranecrosis"), believed to be based upon the so-called denaturation of cell proteins. It is possible that changes of this nature may also underlie neural excitatory processes in living tissue (Nasonov and Aleksandrov, 1940), as is suggested more particularly by the work of Mirsky (1936) on retinal photochemistry. One may therefore suggest that the process of excitation in any biological system involves complex reversible changes at the level of microstructure.

If excitation in neurones actually involves alteration in cellular structure, and if thresholds of sensitivity relate to thresholds of neural excitability, we can regard sensitivity thresholds as points of transition between states of physiological rest and states of reversible

structural modification, i.e. as thresholds of reversible functional change. Hence sensitivity, interpreted as the capacity to perceive an external stimulus, is to be viewed as the outcome of a process in which neural activity is intensified through a reversible destruction of its own microstructure.

In the light of these considerations, we wish to suggest that both sensitivity and neuronal strength may be subsumed under the more general concept of "reactivity." This property may be said to characterize the integral function of any living tissue, being defined at one extreme in terms of its threshold of excitability and at the other in terms of its limit of effective working capacity (Pavlov's "limit of excitation"). "Reactivity" thus denotes the general function of an excitable substratum; it determines the course of its reaction for any adequate stimuli, beginning with the minimal response at threshold level and ending with the maximal strain on working capacity which is followed by a phase of protective inhibition. The concept thus embraces both the sensitivity of the system and its endurance in the face of adverse conditions of stimulation.

V. Conclusion

This paper is concerned with the inter-relationship between the strength and the sensitivity of the nervous system. Comparative data are adduced relating on the one hand, to levels of auditory and visual sensitivity and, on the other, to the strength of nerve cells (in Pavlov's sense of the term) in corresponding regions of the central nervous system. Strength, as so defined, has been ascertained by the use of a method involving a conditioned decrease in sensitivity to light or sound. A negative correlation has been found between the characteristics of sensitivity and strength of the nervous substratum; thus high sensitivity is combined with weakness, and low sensitivity with strength, of the underlying nervous processes. In conclusion, the authors attempt to relate the parameters of strength and sensitivity in terms of a single typological property.

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BINOCULAR VISION WITH TWO STABILIZED RETINAL IMAGES

BY

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Experiments are described in which each eye is presented with a target whose image remains on the same part of the retina when the eye moves. The patterns presented to each eye may be similar and may be placed on corresponding parts of the retina or may be placed in non-corresponding positions: alternatively, different targets may be presented to the two eyes. Each pattern fades intermittently. Sometimes both are seen together and sometimes both fields are dark at once. There is a small negative correlation between the times of clear vision with the two eyes. When corresponding areas of the two retinas are illuminated with red and green light respectively, the composite colour (yellow) is never perceived with steady illumination. When two similar patterns are in nearly corresponding positions there may be subjective fusion. With two different targets there is sometimes a subjective impression that the two patterns move with respect to one another even though their positions on the retina are fixed.

Introduction

In an earlier paper (Ditchburn and Pritchard, 1956) we described an apparatus for producing a visual target whose image remains on the same part of the retina even when the eye moves. Such an image is called a *stabilized retinal image.* The apparatus is shown in Figure 1(*a*) below. The subject wears a tightly fitting contact lens carrying a stalk on which is mounted an optical system which produces an interference pattern (Fig. 2(*b*)). The pattern is located at an infinite distance and an image of the pattern is formed on the retina of the unaccommodated eye. This image is fixed on the retina provided that the angle between the optic axis of the crystal and the visual axis remains unchanged. Riggs, Armington and Ratliff (1954) have investigated the possibility of movement of the contact lens with respect to the eye. They conclude that the natural movements of the eye do not significantly move the contact-lens (except possibly a few of the largest movements). This problem has also been studied by several workers in this laboratory. The results, which will be published elsewhere, support the conclusions of Riggs *et al.* It also appears probable that movements of the retinal image due to micro-fluctuations of accommodation in the emmetropic eye are too slow to affect the results of our experiments. Variations of retinal luminance due to pupil fluctuations (Hippus) are also small provided the apparatus is well adjusted. In the earlier experiments one eye viewed a stabilized image while the other was occluded. It was found that:—

(*a*) the pattern was initially seen very clearly;

(*b*) after a few seconds it faded to a grey field;

(*c*) after another short interval the field went black and remained so for up to 10 sec.;

(*d*) following a convulsive movement of the eyes the pattern was seen again very clearly. The whole cycle was repeated again and again.

The convulsive movement, which is larger than any natural movement, is probably sufficient to cause mechanical destabilization. Sometimes there is partial regeneration not associated with any unnatural convulsive movement. (We have considered

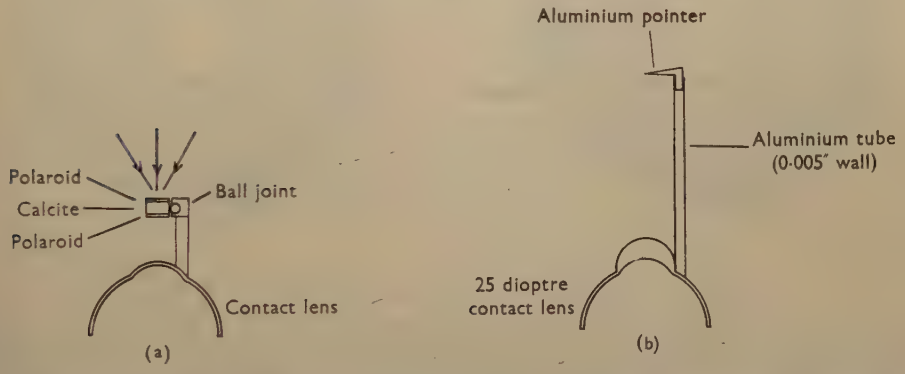
the possibility that this partial regeneration is due to a small mechanical destabilization associated with natural saccadic movements and have recorded the relation between time of flick and time of regeneration. The analysis, which is rather complicated does not indicate a simple causal relation between flicks and regeneration.) The results (*a*), (*b*), (*c*), (*d*) may be compared with other experiments on the stabilized image. (Ditchburn and Ginsborg, 1952; Riggs, Ratliff, Cornsweet and Cornsweet, 1953; Ditchburn and Fender, 1955; Cornsweet, 1956; Fender, 1956; Krauskopf, 1956; Pritchard, 1958*a* and *b*; Ditchburn, Fender and Mayne, 1959.)

It has been suggested that possibly the alternate fading and regeneration represented a form of retinal rivalry between the eye which is seeing the stabilized image and the occluded eye. If this were so then fading to a completely black field should not occur when both eyes are viewing a stabilized image. The present experiments were designed to test this point as well as to provide additional information on the possible location in the visual pathways of the effects produced by the compensation of retinal image motion.

Experimental Methods

The apparatus shown in Figure 1 (*a*) produces the pattern illustrated in Figure 2 (*b*). When white light is used to illuminate the system the rings are coloured, blue being on the inside of each ring and red on the outside. The cross is achromatic (dark grey). The angular diameters of the rings are approximately 5, 7½, 9, 10, 11 . . . degrees. None of the rings are sharp and only the first five are seen clearly. The edge of the field is grey and the diameter is 30° in the visual field. By adding a quarter-wave plate (correctly orientated) between each Polaroid and the crystal, the cross is removed, and the ring pattern obtained alone as is shown in Figure 2 (*a*). The crystal used gave a yellowish white centre.

FIGURE 1



A pattern with sharp boundaries is obtained using the apparatus shown in Figure 1 (*b*). A power of 25D is incorporated in the contact lens. An aluminium tube holds the pointer shown in Figure 2 (*d*) approximately in the focal plane of the lens, so that the subject sees an image at his far point. This is in focus for the unaccommodated eye. The angular size is as shown in the diagram.

With either of the arrangements shown in Figure 2 (*a*) or 2 (*b*) the subject can adjust the position of the centre of the pattern by rotating the crystal about the ball joint. This joint is filled with wax which is soft enough to permit movement with the hand but sufficiently stiff to hold the system in position once it has been set. Thus, if one pattern of this type is used, the subject can bring its centre to his (subjective) visual axis. If a second one is placed on the other eye, the subject can bring the two into a position which he judges to be perfect alignment or into any desired degree of misalignment.

The experimenter can adjust the "pointer" shown in Figure 2 (*d*), so that the point is seen on the subjective axis of vision. If the pointer is seen by one eye and the interference pattern by the other, it is possible to set the point so that it appears to be the centre of

the ring system, Figure 3 (*a*), or in any other desired relation to the ring system (e.g. as shown in Figure 3 (*b*)). When two ring patterns are presented both crosses may be similarly oriented or the apparatus may be adjusted so that the orientation of the cross seen by the left eye is at 45° to that seen by the right eye. When the patterns are set so that the rings appear to be superposed, the combined pattern presented to both eyes is that shown in Figure 2 (*c*).

FIGURE 2

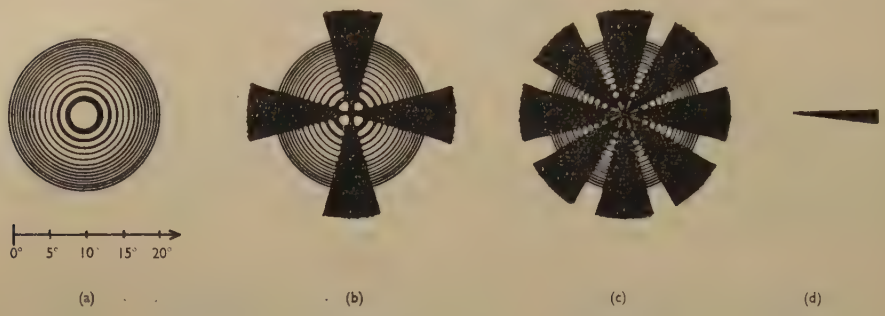
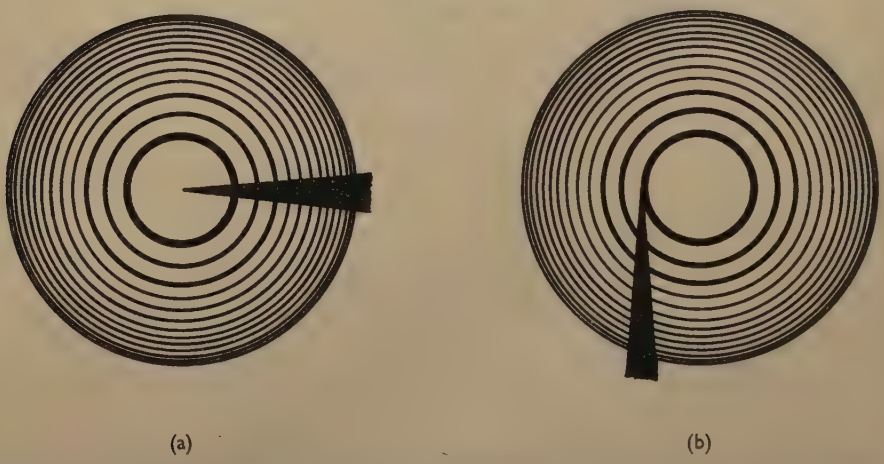


FIGURE 3



The optical systems used in the present experiments to produce stabilized images, are illuminated by magnified images of the ribbon filament of a 6 volt 108 watt D.C. lamp. The subject rests on a couch, the back of the head and shoulders being supported by a loosely fitting plaster cast. He is provided with two press-keys with which he can signal the appearance and disappearance of each image individually. These signals are registered on a three pen recorder, the third pen of which records half seconds from a standard timer.

The parameter measured in the present experiments is the Visibility Factor (V.) defined as:

$$\frac{\text{Time for which some image structure is seen}}{\text{Total viewing time}} \times 100 \text{ per cent.}$$

Results

(*a*) *Discomfort*

It has previously been noticed that subject report considerable fatigue and strain when viewing a single stabilized image for periods of about an hour in all, even though a rest is allowed every 2 min. This feeling of fatigue and strain is very much more

serious when the subject views two stabilized images. It occurs under all conditions but is worst when two patterns are misaligned by a small amount (approximately 1°). If the patterns are well aligned, or are very much misaligned and the subject accepts the situation, the arrangement is still unpleasant but tolerable. In normal vision occlusion of one or both eyes for a short period does not produce any feeling of fatigue or strain. The perceptual situation involved in viewing a stabilized image thus differs from normal vision in other respects besides the reduction of information.

(b) Measurement of retinal rivalry

The experiments described in this section were carried out with two targets aligned in the way shown in Figure 2(*c*). It is thus possible to distinguish the target presented to the left eye from that presented to the right eye. The subjects report considerable periods when the retinal images appear to fade alternately in the two eyes. However, periods do occur when both images disappear together and there are other periods when segments of both images are simultaneously visible. It thus appears that we have neither of the two extremes—complete alternation or complete independence. This is shown by analysis of Table I which summarizes data from records obtained when the subject used two keys as explained above. S_L and S_R are stabilised visibility factors for left and right eye respectively and P_M is the percentage of time for which at least one target is *not* seen. If there were complete alternation between the eyes we should have $S_L + S_R = 100$ and $P_M = 100$. P_M is always less than 100 (usually much less) and $(S_L + S_R)$ is substantially greater than 100 in 9 out of 12 experiments. We therefore conclude that, except possibly with weak blue light, we do not observe complete binocular alternation.

TABLE I

Filter		*Luminance*		*Visibility Factor*			*% time for which at least one eye is not seeing*	
Left eye	*Right eye*	*Left eye*	*Right eye*	*Left eye*	*Right eye*	S_L+S_R	*Measured* P_M	*Calculated* P_C
		ml.	ml.	%	%		%	%
White	White	3·5	1·0	74	79	153	44	42
,,	,,	,,	,,	80	82	162	34	34
,,	,,	,,	,,	72	83	155	44	40
Green	Green	1·0	0·1	77	74	151	48	43
,,	,,	0·2	0·1	64	70	134	65	55
Red	Red	,,	,,	56	72	128	71	40
,,	,,	,,	,,	69	65	134	66	55
Blue	Blue	,,	,,	47	44	91	87	79
,,	,,	,,	,,	55	55	110	73	70
,,	,,	,,	,,	57	43	100	74	64
,,	,,	1·0	0·5	62	75	137	57	53
,,	,,	,,	,,	74	62	136	62	54

The coloured patterns were obtained with the Chance filters OG.1; OR.1 and OB.10 and the luminances stated were measured with a flicker photometer.

If, on the other hand, the eyes were acting entirely independently the percentage of time for which both targets are seen would be $S_LS_R/100$ and the percentage of time for which at least one image is not seen would be $P_C = (100 - S_LS_R/100)$. The measured value (P_M) is obviously more than the calculated (P_C). This shows that

when the target presented to the left eye is not perceived the probability that the target for the right eye is also not perceived is less than it is when the target presented to the left eye is perceived. Thus there is some degree of interaction between the eyes.

The fact that there is some interaction but not a complete alternation is well established and is in accord with subjective impressions. It is, however, not justifiable to draw conclusions from the small differences of S_L and S_R for the different conditions used. The results are affected by the fact that the decision to signal that a target is not seen has a tendency to cause regeneration of one or both images. For both subjects the right eye is the dominant eye. The luminances were adjusted to make the values of S_L and S_R roughly equal.

(*c*) *Observations in the "black field"*

As stated in the Introduction, the field from time to time goes completely dark. Sometimes this happens sharply and then it always happens in both eyes simultaneously. It also ends suddenly in both eyes when the subject has the impression of a convulsive movement. In earlier experiments subjects have sometimes reported that when the target region "went black," weak unstabilized images either in the same eye or in the other eye also disappeared. It seems very probable that this general failure of visual perception is central rather than peripheral.

(*d*) *Misaligned targets*

As stated above, the subject can adjust the patterns so as to produce what he judges to be perfect coincidence of the centres. It is unlikely that his control gives an adjustment of much better than 30 min. of arc. The subject is thus unable to detect a misalignment of about 20 min. of arc.

When the targets are misaligned by about 1°, the subject can no longer fuse them. He attempts to do so and makes irregular and unco-ordinated movements of 10°. He reports the situation as painful but is unaware that such large eye movements are occurring. There are, of course, no corresponding movements of the retinal images. When the targets are misaligned by 5° the subject is more comfortable and can decide to pay attention to one pattern or the other. If one pattern is now continuously mechanically destabilized by disturbing the contact lens, the other pattern continues to fade and regenerate.

(*e*) *Apparent movement*

When the pattern of rings is seen in one eye and the pointer in the other, it is possible to set the pointer so that it just touches the innermost ring. The subject reports that the relation between pointer and ring does not remain constant over a period of a minute or so. It is possible that this effect is due to a slip of one lens on the sclera but this seems very improbable (for reasons given in the Introduction to this paper). If the eyes are moved deliberately, the relation between ring and pointer alters. This may be due to slip when the voluntary movement is very large but probably not when the voluntary movement is small.

(*f*) *Colour addition*

In normal vision it is found that, under suitable conditions, some observers can obtain additive colour mixing. When, for example, a patch of one retina is illuminated with red light and the corresponding patch of the other retina with green light, the additive colour (yellow) is perceived. We were able to obtain this effect with unstabilized images both with the ring systems and with small patches of light. The additive yellow colour was never observed with stabilized images. Sometimes one

colour was seen alone and sometimes patches of red were seen adjacent to patches of green. In general, when one pattern is illuminated with red and the other with green, the centre is perceived as green with a red surround. If one is illuminated with red and other with blue then the composite pattern has a blue centre. There are also occasions when part of the outer ring system is perceived as red and another part as green. All colours appear desaturated. If intermittent illumination (about 2 c.p.s. with the same phase in each eye) is used, the composite colour is then seen. This is in accord with other experiments which show that if stabilized images are viewed in low frequency intermittent illumination normal visual perception is restored. (Ditchburn and Fender, 1955; Cornsweet, 1956; Ditchburn, 1956; Fender, 1956; Pritchard, 1956*b*).

Discussion

When a subject views a single stabilized image there are periods when the pattern is not seen at all and periods when some part of the pattern is perceived. The latter periods are described as "periods of vision" but it should be emphasized that during these periods vision is not normal. During these times, the image appears rather faint and diffuse generally, part of the pattern may be missing or some parts of the field may appear clearer than others. Sometimes distortions of the pattern may be "seen." These observations cannot be expressed quantitatively but they do show that during the "periods of vision," the complete visual signals associated with normal vision are not being received. The fragmentary signals are being interpreted with difficulty and not altogether correctly. The observations that a considerable degree of misalignment cannot be detected (see (*c*) above) and that the additive colour is not perceived (see (*f*) above) suggest that the brain is unable to combine and compare the signals from the two eyes as it does in normal vision. These observations would readily be explained if the two patterns were not seen simultaneously but we find that, although there is some degree of alternation, there are some periods when both patterns are seen. When one eye receives green light and the other red, the additive colour is not perceived but in some regions of the visual field red is perceived and in adjacent regions green. This suggests that at any one time and place in the combined visual field the signals from one retina dominate to such an extent that signals from the other are excluded from perception. It is easy to understand that if a similar situation exists when two white patterns are presented, accurate detection of misalignment would not be possible. The above interpretation suggests that retinal rivalry may have to be considered in relation to local regions of the visual field as well as for the visual field as a whole.

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SHORTER ARTICLES AND NOTES

A SIMPLIFIED METHOD OF TREATMENT FOR SIZE-CONSTANCY DATA

BY

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The traditional method of expressing the result of a size-constancy experiment is by calculation of the Thouless (1931) index

$$T = \frac{\log P - \log S}{\log R - \log S}$$

where P is the ratio of the "phenomenal" size of the two objects compared, S that of their stimulus, or perspective, sizes, and R that of their real sizes.

While this formula in itself is satisfactory, there are advantages in applying it in the manner outlined below.

(1) The introduction of P is sometimes confusing to students (and others), especially as in any actual experiment it is arranged that there is an equality match between the two apparent sizes: $P = 1$ and $\log P = 0$. It seems better to define T solely in terms of physical quantities which are actually measured, it being understood that the definition applies to the match condition. Thus we may put

$$T = \frac{\log S}{\log S - \log R} = \frac{\log S}{\log \frac{S}{R}}$$

(2) The question of whether T remains constant for different distances of the standard (and/or variable) is not often explicitly discussed. It is commonly taken to do so, within any ordinary range of distances such as those used in a laboratory experiment. Certainly in a student's laboratory exercise or in simple determinations of the index such for instance as those sometimes made in a clinical investigation, it is sound practice to make measurements for various distances of the standard. In such cases it is convenient to plot all the data on a single graph in a way that allows T to be immediately read off, while at the same time its distance-invariance may be directly confirmed, or departures detected.

The following simple procedure satisfies these requirements, and provides a useful exercise for the student in the use of graphical methods.

We may consider, for concreteness, the familiar type of experiment in which a variable diamond form at a fixed distance is compared with another, standard, one of fixed dimensions.

Let A_s be the measured dimension of the standard and A_v that of the variable which is matched with it. Let D_s and D_v be the distances of standard and variable from the subject's eye.

$$\text{Then } S = \frac{A_s/D_s}{A_v/D_v} = \frac{A_s}{A_v} \cdot \frac{D_v}{D_s}$$

$$R = A_s/A_v = y \qquad \text{say}$$

$$S/R = D_v/D_s = 1/x \qquad \text{say}$$

$$\text{and } T = \frac{\log y/x}{\log 1/x} = \frac{\log x - \log y}{\log x}$$

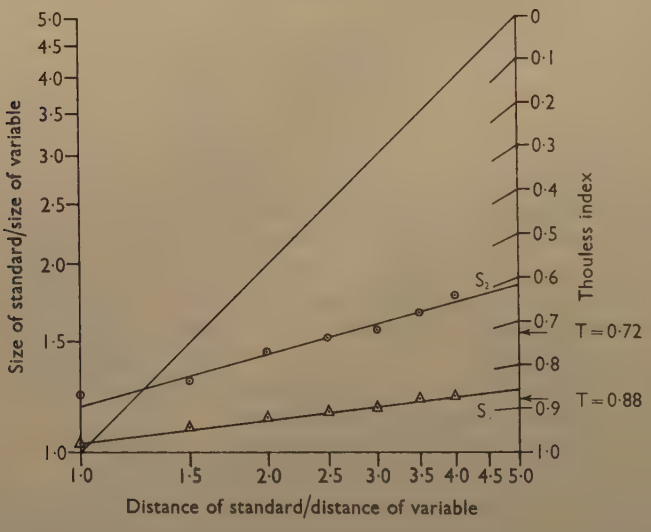
$$\text{whence } \log y = (1 - T) \log x$$

Thus if y, the ratio of the sizes of standard and variable, is plotted on double logarithmic paper against x, the ratio of their distances, the points will lie on a straight line through the origin, provided T is indeed constant for different distances of the variable. Moreover the gradient of this line is $(1 - T)$ and T is immediately determined. The X-axis represents complete constancy ($T = 1$), and the line at 45° through the origin zero-constancy or equal-subtended-angle (perspective) judgments. Points above the latter line correspond with negative-, and below the former with hyper-constancy. Values for positions of the standard nearer to the subject than the variable lie in the left-lower quadrant.

An easy means for measuring gradients of lines through experimental points is provided by erecting an ordinate equal in height to its distance from the Y-axis and dividing it into (say) ten equal parts. The dividing points are labelled T = 0·9, 0·8, 0·7 . . . 0·1 in order upwards from the X-axis. The value of T can then be read from the intersection of the line through the experimental points, provided the latter passes through the origin. If it does not, the parallel line passing through the origin is used.

Figure 1 shows the plot of sets of readings obtained on two subjects.

FIGURE 1



It is perhaps not uninteresting to note that the Thouless ratio, if distance-invariant, carries with it the following very simple implication. If the *apparent size* of an object be defined as the size of a variable object at a fixed distance which is judged equally large, then *apparent size varies inversely as the* $(1 - T)^{th}$ *power of*

the distance. A number of considerations, however, discourage any attempt to regard this relationship as providing a metric for visual space, the most notable being the dependence of T upon the angular separation of the objects compared (cf. Joynson, 1949).

I am indebted to Messrs. P. S. Delin and T. Morris for the data relating to Subject 2.

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THE EFFECT OF LACK OF SLEEP ON VISUAL WATCH-KEEPING

BY

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Sixteen subjects performed a test of visual watch-keeping twice, at the same hour of the same day of successive weeks. The test lasted 40 min. On one of these occasions the test was done after normal sleep the previous night; signals were detected at a steady rate throughout. On the other occasion the subjects had no sleep the previous night; they passed the time in innocuous leisure activities. Here a similar level of performance was maintained for the first 20 min. of the test. After this the rate of signal detection declined steadily. In the final quarter of the test the rate was roughly one third that of the first quarter.

Signals missed were divided into three categories, (*a*) signals missed while watching the display, (*b*) signals missed while not watching, and, (*c*) signals missed while asleep. Lack of sleep produced increases in all three of these categories.

These results support previous suggestions that a test has to be prolonged before performance is affected by moderate loss of sleep; they also suggest that it is the simpler, or perhaps the more predictable, tests which are more affected.

Introduction

In the past a number of studies have been made of the effects of lack of sleep on performance; as a rule periods of deprivation have ranged from 48 to 96 hr. In very few of these has loss of sleep been found to cause substantial and statistically reliable changes in performance. One reason for this may be that experimenters have tended to use relatively complex tasks, prompted, perhaps, by too literal an interpretation of Kleitman's (1939) suggestion that the main effect of lack of sleep is probably one of "fatigue of the higher levels of the cerebral cortex." Further, tests have rarely been prolonged for more than a few minutes. The result is that we have abundant evidence to show that short, complex tasks are little affected by loss of sleep even when severe.

In the present experiment a completely different type of test was used, a prolonged, simple one, that of visual watch-keeping.

Method

Sleep-deprivation

An invariable routine was observed by groups of subjects undergoing sleep-deprivation. After a normal day of duties and leisure the subjects assembled in the recreation room of the naval unit which was accommodating them. They remained there, under supervision, throughout the night, passing the time talking, writing, playing cards or table tennis. Little restriction was placed on them providing they remained awake. At 7 a.m. they performed their usual morning offices, had breakfast, and then walked about one mile to the laboratory, where they were tested at times varying from 8 a.m. to 12.30 p.m. Those tested later in the morning performed routine duties at the Naval Unit, under supervision, until due to leave for testing. Subjects who had normal sleep had exactly the same routine, except that between the hours of 11 p.m. and 7 a.m. they were free to take a normal night's sleep. The comparison is not, therefore, strictly between subjects who have had no sleep for 4 hr. and subjects deprived of sleep for 26 to 30 hr. Administrative obstacles made it impossible to supervise the non-sleepers throughout the preceding day until the 11 p.m. assembly, and so a full 26 to 30 hr. sleeplessness cannot be guaranteed. In fact, interrogation of the subjects suggested that no attempt was made to store sleep

on the preceding day. Nevertheless, the categories of sleepers and non-sleepers can more accurately be described as representing, respectively, subjects who had normal sleep and those who had no sleep during the night previous to testing.

Subjects

Subjects were naval ratings mostly between the ages of 18 and 30. All were volunteers to stay awake.

The test

The present study used one basic task which for future reference will be termed the Standard Vigilance Test. The subject sat in a cubicle and watched a ground glass screen 12 in. in diameter and 6 ft. away for the very occasional appearance of a small spot of light only slightly brighter than the back-illumination of the screen; it was approximately $\frac{3}{8}$ in. in diameter and appeared on the screen for $\frac{1}{2}$ sec. It could appear in any of eight positions equidistant on an imaginary circle of radius 4 in. from the centre of the screen. This spot of light was called the Signal; when he saw it the subject had to report the fact by pressing a morse key. The test lasted 40 min. during which the subject sat and (ideally) watched the screen continually; during this time 16 Signals appeared, irregularly spaced in time, but so arranged that four of them occurred in each quarter of the test. Spatially they occurred at random in any of the eight possible positions on the screen.

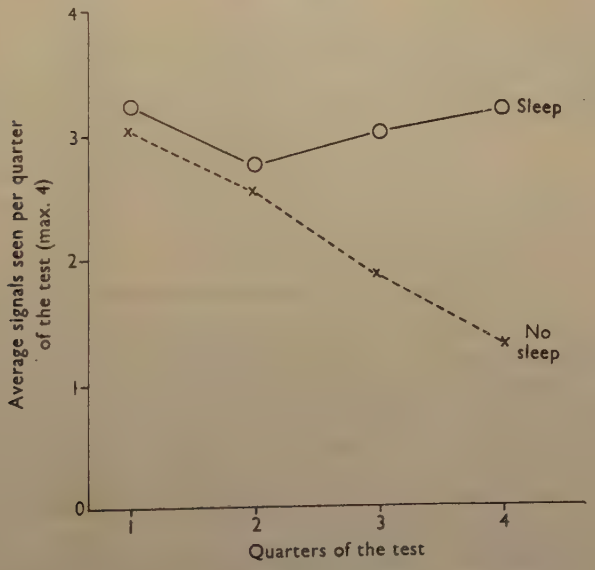
The subject wore headphones; through them he heard "white noise" at a level of about 70 to 80 db. throughout the test. This served to reinforce the sound-proofing of the cubicle in that it masked structured extraneous noise from outside. The experimenter watched the subject, without his knowledge, through a one-way window during most of the test.

On the day preceding the first day of testing the subjects were given the full instructions followed by a short run to make them familiar with the test. On the following day, before the main test, the subject was reminded of these instructions and his memory refreshed, if necessary. He was told that the test lasted 40 min., but was relieved of his watch so that he had no check on the passage of time.

Experimental conditions

In this experiment the Standard Test was given to sixteen subjects, once after sleep (henceforth S) and once after no sleep (henceforth NS). Both tests were performed at the same hour of the same weekday, one week separating the two. Two different programmes of Signals were used, one for each test. The design was arranged to balance possible

FIGURE 1



The effect of lack of sleep on number of signals seen.

differences between performance on the first and on the second tests due to "practice" effects, and also between performance on the two programmes of Signals, which could not be assumed of equal difficulty.

Results

Here we shall be concerned with the degree to which performance deteriorated in relation to normal levels as a result of loss of sleep; this will be briefly referred to as the "NS decrement." With S, subjects saw an average of 11·9 Signals out of the total of 16 presented during the test; with NS they saw an average of 8·9 Signals. This difference is significant at the 5 per cent level. (See Table I (Overall Scores).)

However, almost all of this NS decrement in total score for the whole test was due to sleepless subjects failing to maintain normal levels of performance in the later stages of the 40-min. test. (See Fig. 1.)

An index of this deterioration of performance during the course of the test is given by $Q = 2(P_1 - P_4) + (P_2 - P_3)$ where P_1, P_2, P_3, and P_4 are totals of Signals seen in the 1st, 2nd, 3rd, and 4th ten-minute periods of the test, respectively. When this index is used as a basis for an analysis of variance the NS decrement is significant at better than the 1 per cent level. (See Table I (Q scores).)

TABLE I

Analysis of Variance of Vigilance Results using $\sin^{-1}$ Transformation of Scores of Signals seen in each Quarter of the Test

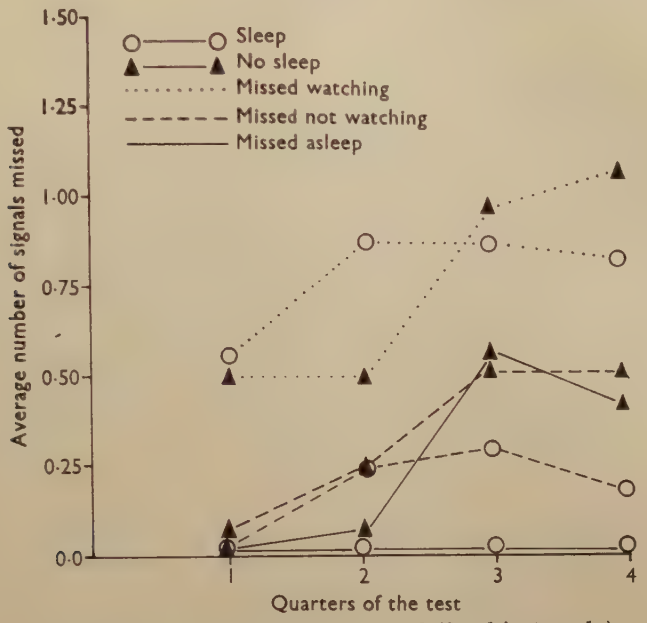
		Overall test score		*Within-test deterioration score* $Q = 2(P_1 - P_4) + (P_2 - P_3)$			
Source	*d.f.*	*Mean Sq.*	*F*	*Mean Sq.*	*F*	*Test agst.*	*P equals:* 0·05 0·01
Subjects							
Teams (T) (or S *vs* NS × D)	1	3·45	0·43	0·3	0·02	WT	4·60 8·86
Within Teams (WT)	14	8·06		19·9			
Conditions							
Sleep *vs.* No Sleep	1	33·01	6·64*	236·5	25·00**	WT × S *vs.* NS	4·60 8·86
Days (D)	1	17·26	3·47	0·1	0·00	WT × D	4·60 8·86
Residual							
WT × S *vs.* NS (or WT × D)	14	4·97		9·5			
Total	31	7·62		20·9			

But what was happening when subjects missed Signals? The experimenter recorded his estimate of the subject's attitude, as each Signal was missed, under one of three heads, (*a*) watching the screen, (*b*) not watching, with eyes shut or looking elsewhere, (*c*) not watching, asleep (eyes shut for at least 1 min. previous to the appearance of the Signal). Unfortunately these observations were taken only on the last eight subjects tested (who, nevertheless, formed a balanced group). These records have been analysed in Figure 2 to contrast the way in which subjects missed Signals in the S and in the NS Conditions.

It would appear from this that at least part of the inefficiency of the sleepless was due to their falling asleep or not looking at the screen. Nevertheless towards the end

of the test they also missed more Signals while actively watching the screen, when presumably the Signal fell on the retina but failed to evoke a response.

FIGURE 2



Analysis of how signals were missed (8 subjects only).

Discussion

When this experiment was completed and briefly reported four years ago Wilkinson (1955), it was the first instance of a large and significant effect of as little as 30 hr. loss of sleep. Since then the susceptibility of simple, prolonged tests has been confirmed by Pepler (1958) and Wilkinson (1958) for the same term of sleep deprivation, and by Williams, Lubin and Goodnow (1959) for longer periods. To the extent that it is simple, repetitive, and lacking in feedback of knowledge of results the test may be described, to use Hebb's term, as lacking in cue factor. As it is prolonged the task may become less novel and therefore be less likely, to use a mixture of the ideas of Berlyne (1950) and Broadbent (1953), to compete with other features of the stimulus totality for the subject's response.

This examination of the nature of the test suggests why it comes to be so much affected by lack of sleep. The unstimulating nature of the test and its environment may interact with the depressant effect of the stress to produce lowered level of arousal culminating, as we have seen, in overt sleep in some subjects. This trend may cause changes in the organization of the central nervous system of a kind reminiscent of Jacksonian dissolution. One result of these changes according to Jackson (1932) is that more automatic, less voluntary modes of behaviour are favoured, and this means that a task lacking in novelty and no longer exciting the subject's curiosity is less likely to be attended to. From our description of the vigilance test it would appear that it is just such a task, especially in the later stages of the test which is when we find the main impairment. To sum up then, this test may be affected so much by lack of sleep because its environment encourages lowered arousal or dissolution and its nature renders performance of it particularly susceptible to the influence of this change in central organization.

This work was prompted by a suggestion of Dr. N. H. Mackworth, it owes much to advice from him and from Mr. D. E. Broadbent. Thanks are due to the Navy for providing subjects and facilities.

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DISCRIMINATION LEARNING THEORY: EXCITATORY vs. INHIBITORY TENDENCIES IN MONKEYS

BY

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Two methods of testing between excitatory and inhibitory tendencies in simultaneous two-choice discrimination learning by monkeys are proposed. The use of both methods yields results favouring an excitatory as opposed to an inhibitory mechanism as the unitary process in discrimination learning.

Introduction

In an important recent paper Harlow and Hicks (1957) present evidence in favour of the uniprocess conception of simultaneous two-choice learning in monkeys. They also consider what the nature of this unitary mechanism might be. "There are three possibilities: the mechanism may be excitatory, inhibitory or undefined—a mechanism X. Wisdom or cowardice would dictate mechanism X. Hebb apparently prefers excitation, and the authors prefer inhibition. At the present time there appears to be no direct test among the three mechanisms, and, this being true, choice is dictated merely in terms of which thesis best integrates existing learning data" (pp. 108–109). The present author is both wanting in wisdom because he prefers excitation and also in prudence because he ventures to propose two relatively direct methods of testing between the alternatives, excitation and inhibition.

He holds, in conformity with others, that an excitatory tendency would accrue from reward, and thus also from approach to the positive cue, inhibition from non-reward and approach to the negative cue. However, it is also suggested that if excitation underlies the process of discrimination learning then approach to the positive cue rather than non-approach to the negative cue would be the response that has been learnt and is manifested during successful activation of the formed habit; that if inhibition is basic in the process of learning then non-approach to the negative cue would constitute the final habit. Should this view be accepted then it is possible to test between excitation and inhibition by examining whether an animal approaches the positive cue or avoids the negative cue during consistently successful performance in the two-choice situation. Two ways of making this determination are proposed. In the more direct method the animal's movement leading to *correct* choice in a somato-sensory discrimination are carefully observed: the number of times tactile examination of the positive cue is followed by deliberate palpation of the negative cue prior to choice is taken as an index of the tendency to avoid the negative rather than to approach the positive cue. This principle could be adapted for visual testing. There is, however, no reason known to the author to suppose that the mechanism in two-choice discrimination learning is not the same for all modalities. In the second method a comparison is made between the increase in errors when either the positive or negative cue is spatially separated from the site of response and reward.

Method I and Results

Monkeys are most conveniently trained on a somato-sensory discrimination in a standard Wisconsin-type General Training Apparatus (Harlow and Hicks, 1957) from which all light is excluded during trials. The animal's movements can still be watched

by means of an infra-red observation device (Ettlinger and Wegener, 1958). It may then be seen that in some instances the animal's final choice is directed towards the cue it touched first without palpation of the alternative cue; on other trials it selects the original cue only after tactile examination of the second cue; or the animal may select the alternative cue after shifting from the one it touched first. As a rule discrimination training is continued until the animal makes no more than 10 errors in 100 successive trials. In such blocks of 100 relatively successful trials as at all stages of training, the animal will first chance to touch the positive rather than the negative cue in one half of all trials (on the average). Neglecting the 10 incorrect choices in such blocks, the animal will not need to shift and feel the negative cue before making the response appropriate to the positive cue and gaining the reward if it has learnt to approach the positive cue. If, however, the animal has learnt to avoid the negative cue, it might be expected to shift from the positive to the negative cue and then revert once more to the positive cue for the final response and reward.

The comparison behaviour shown by four unoperated rhesus monkeys (C1–C4) on each of two somato-sensory two-choice discrimination tests has been observed and recorded. Here reference is made only to observations taken during the final 100 trials on each test (when not more than 10 errors were made). All animals had previously been trained to the standard level of performance on at least six visual or somato-sensory discrimination tasks, but not on spatial alternation. Then they all learnt to discriminate between an hexagon and a circle in the dark; subsequently the shapes L and T served as alternative cues in a further somato-sensory test for two animals while two squares of unequal surface area served similarly for the other two monkeys. The details of the training histories of these animals and of the tests are to be published elsewhere.

When the records for these four animals are examined and all incorrect responses have been excluded from the final 100 trials on each of two tests it is found that the negative cue was touched at some time during the trial in 464 of 720 trials. Chance initial palpation of the negative cue is expected in 360 of 720 trials. The 464 examinations of the negative cue significantly exceed the chance expectation ($\chi^2 = 59{\cdot}6$, $p = 0{\cdot}001$). Nevertheless a correct choice was made in 256 trials *without* touching the negative cue. This would imply that the animals have learnt to approach the positive cue without essential reference to the negative cue. They did make deliberate reference to the negative after touching the positive cue in an estimated 104 trials. It may be hazarded that deliberate reference to the negative cue might have been made less frequently if the animals had reached a standard of discrimination more efficient than 90 per cent correct in 100 trials.

Method II and Results

Monkeys can become proficient on a two-choice simultaneous discrimination test even though the reward containers are covered by identical lids and the alternative cues are adjacent to each container. Subsequently either the positive or the negative cue can be separated by a small distance from its container. Such spatial separation of cue from response and reward decreases the efficiency of discrimination. Now if the animal has learnt to approach the positive cue then separation of the positive cue from the container should lead to greater decrement in efficiency than separation of the negative cue. If, however, it has learnt to avoid the negative cue it might be predicted (although perhaps with less confidence) that separation of the negative cue will give rise to more errors than separation of the positive cue.

A number of rhesus monkeys, including the unoperated animals C1–C4, learnt such a "spatial" discrimination as their third or fourth test. In this discrimination the

cues (three-dimensional objects) were placed on rails connecting the two reward containers. These were set 12 in. apart (centre to centre) and were covered with identical lids. Initially each cue was immediately adjacent to its container. Following completion of other testing, some of the animals reverted to this task and were retrained to the standard level of performance (not more than 10 errors in 100 successive trials). Then three new test conditions were introduced, each for only 100 trials, in a different order for different animals. *Either* (*a*) both cues were separated by 1 in. from their container (being moved 1 in. towards the other container); *or* (*b*) the positive cue alone was separated by 2 in. (again towards the other container); *or* (*c*) the negative cue was separated by 2 in. from its container. The left/right position of the positive cue and reward was, of course, always varied according to a balanced schedule. If an animal made more than 10 errors during the 100 trials of a new test condition ((*a*)–(*c*)), training was continued under the original conditions (both cues adjacent to containers) until no more than 10 errors had been made in the preceding 100 trials.

In Table I are shown the error scores for all animals tested under the new conditions. Animals C1–C4 are unoperated and served as a control group in another study (to be reported elsewhere). Following 100 trials under condition (*a*), animal C3 was not able to regain the standard level of performance within 500 trials when tested under the original conditions. Animals P2 and T1 had previously received bilateral ablations of the posterior parietal and inferior temporal regions. Three animals were tested in the light, the remainder under identical conditions in the dark. The figures in brackets represent the order in which the various test conditions were applied. In the case of two animals it was considered desirable (and it proved practicable) to increase the separation of the cues from the containers since only small differences were obtained between performance under conditions (*b*) and (*c*).

TABLE I

EFFECT OF SEPARATION OF CUES FROM SITE OF RESPONSE AND REWARD

Animal	*C2*	*C3*	*P2*	*C1*	*C4*	*T1*
	Visual modality			*Somato-sensory modality*		
Separation of cues from containers:—						
(*a*) Both by 1 in.	16 (2)	37 (1)	14 (1)	2 (3)	9 (1)	3 (2)
(*b*) Positive by 2 in.	23 (3)	—	9 (2)	9 (2)	27 (2)	4 (1)
(*c*) Negative by 2 in.	7 (1)	—	2 (3)	6 (1)	3 (3)	2 (3)
Positive by 4 in.				21 (5)		10 (4)
Negative by 4 in.				0 (4)		5 (5)

The figures not in brackets represent the number of errors made in 100 trials under a given condition of cue separation. The figures in brackets indicate the order of testing under different conditions for each animal, where (1) indicates first in order, (5) indicates fifth.

The error scores can not be evaluated statistically because of the small numbers of animals in each group when differing operative status is taken into account. Only animals C1, C2 and C4 are legitimately comparable, and also contributory, since C3 was not tested except under condition (*a*). However, inspection of the Table shows that more errors are made *in every instance* when the positive than when the negative cue is separated from the container—irrespective of the order of testing. This would suggest that both in the dark and the light the animals have learnt to approach the positive rather than to avoid the negative cue.

Conclusions

The findings based on two quite different methods of comparison therefore tend to agree that approach to the positive cue is learnt as a consequence of discrimination training. As has already been suggested, this result would lead the author to infer that excitation, as opposed to inhibition, is the underlying mechanism in this kind of learning. It must, however, also be acknowledged that mechanism X of the wise, once it has been divested of its anonymity, may yet come to find support in the present findings.

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VERY BRIEF DELAY OF IMMEDIATE RECALL

BY

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In a series of experiments, Brown (1955, 1958) reported a number of important suggestions concerned with the effects of interfering with immediate recall. His results which are relevant to this Note can be summarized briefly as follows:

(*a*) A very brief delay due to interpolating a single stimulus between the end of a message and the beginning of the recall of it, led to a small memory loss.

(*b*) When the interpolated material consisted of five pairs of digits, the memory loss was very much greater.

(*c*) When the interpolated material (three pairs of digits) was delayed so that some time was available for rehearsal after the message had been presented, recall improved significantly with longer delay of the interpolated material.

The above results were obtained when in general the following conditions held:

(*a*) The message to be recalled was within the immediate memory span for most of the subjects used.

(*b*) Subjects could not predict what the interpolated material was going to be.

Conrad (1958) showed that when the length of message was such that the probability of correct recall was of the order of about 0·6 to 0·7, interpolating a single digit before recall, led to a very large memory loss, even though the nature of the interpolated digit was known to the subjects in advance. The present Note briefly describes a number of experiments in which the original message was beyond the memory span for the subjects used, and the interpolated material known. These experiments complement Brown's and add a little more to what is known about the effects of delaying recall.

The general procedure was the same for all experiments. The subjects were Naval Ratings who listened to messages consisting of seven or eight digits which had been recorded on magnetic tape at a rate of 120 digits per minute. At the time appropriate in each experiment subjects repeated back verbally what they could remember of the message. This was written down by the experimenter and scored right or wrong. For a message to be scored right, the correct digits had to be reproduced in the correct order.

The digit messages were derived from tables of random numbers with the constraints that the digit nought was not used, no digit occurred twice in a message and obviously easy sequences were avoided.

Experiment I

Procedure

Sixteen subjects listened to 8-digit messages, arranged in two blocks each of ten messages. There were two conditions and although each subject heard all twenty messages in the same order, the instructions associated with the two blocks were alternated between subjects. Conditions were therefore not exclusively linked to a particular set of messages. In one condition, subjects were told to repeat back what they had heard immediately. In the other condition, they were again to repeat back what they heard, but always preceded by the digit o. Thus the correct response to a message 74192863 would be 074192863.

Results

Without delay, the mean number of correct recalls was 72·5 per cent. With delay, the mean number was 37·5 per cent. No subject recalled more messages correctly with than without the delay.

Experiment II

Procedure

This was similar to that of Experiment I. There were three conditions, each consisting of a block of twenty 8-digit messages, and fifteen subjects different from those previously used did all conditions in a balanced order. The conditions were:

(*a*) Recall required immediately.

(*b*) Recall required after a 5-sec. interval during which the subject was urged to rehearse.

(*c*) As (*b*) but response always immediately preceded by the digit 0.

Results

Condition (*a*) 61·3 per cent. correct recalls.

Condition (*b*) 57·5 per cent. correct recalls.

Condition (*c*) 23·8 per cent. correct recalls.

No subject did better in condition (*c*) than in either (*a*) or (*b*). Comparing conditions (*a*) and (*b*), 8 subjects were better in (*a*), 6 were better in (*b*), and there was one tie. It seems reasonable to suppose that conditions (*a*) and (*b*) were not significantly different at any acceptable confidence level, but that (*c*) was in fact significantly worse.

Experiment III

Experiment I showed that very brief delay could lead to a considerable memory loss. Experiment II showed that with sequences of this length, little or no consolidation occurred when subjects were given the opportunity to rehearse. This suggested that when a rehearsal interval is provided, a delay at any point during the interval will lead to memory loss.

Procedure

Three conditions, each using a block of thirty 7-digit messages. Eighteen different subjects did all conditions in a balanced order.

Condition (*a*). Recall required after 10 sec. for rehearsal.

Condition (*b*). The subject spoke the digit 0 immediately after hearing the message, then followed 10 sec. for rehearsal before recall was required.

Condition (*c*). Recall preceded by the digit 0, required after 10 sec. for rehearsal.

Results

Condition (*a*) 70·0 per cent. correct recalls.

Condition (*b*) 44·3 per cent. correct recalls.

Condition (*c*) 49·5 per cent. correct recalls.

Only one subject did better at condition (*c*) than (*a*) (2 ties), and none did better at (*b*) than (*a*) (2 ties). Scores on conditions (*b*) and (*c*) were compared by means of the Wilcoxon matched-pairs signed-ranks test (Siegel, 1956). No evidence was found which could justify rejection of the hypothesis that the scores did not differ.

Conclusions

It seems likely that the most important difference between these experiments and those of Brown is in the length of message used. No subject could correctly recall

immediately ten successive 7- or 8-digit messages. Under these conditions—when subjects may or may not get the message right—a delay (or interference) of perhaps less than one second appears to be very damaging to the trace.

The effects of rehearsal also seem to be different according to whether the message length is within or outside the memory span. With our longer messages, rehearsal did not lead to improved recall. Nor did rehearsal appear to prevent the damaging effects of a delay before recall but after the rehearsal period. Indeed it seems immaterial whether the delay comes before or after the rehearsal.

In view of the doubt which currently prevails concerning the true cause of the forgetting that occurs in these conditions, and considering the brevity of the delay required to produce it, one feels perhaps that it is more useful to talk of an interruption rather than of a delay, since this suggests both a time duration and an interference. It is though, evident that in these experiments rehearsal has little or no effect of strengthening the trace, whereas with the shorter messages used by Brown, some consolidation did occur. In neither case could subjects know whether each rehearsal (assuming a series of formal mental repetitions) was correct. But one might imagine that with short messages, successive mental repetitions would be identical, and that the first was likely to be correct. There are two hazards with longer messages. First, repetition is more likely to be wrong, and secondly, if the subject is unaware of the error, the chance of subsequent repetitions becoming right would be small.

What seems to be of most immediate importance is to try to clarify the nature of the interruption that can destroy a just established memory trace. Clearly, as many experiments in this field have shown, interpolated tasks using material of a not very dissimilar kind from that of the message being remembered, can produce forgetting. This has led to theories both of interference and of decay, since interpolation takes time. Strictly, the experiments reported here go no further. But if the concept of interruption of the consolidation process is accepted as a postulate in forgetting, then we need to consider what kinds of event can interrupt in this way. We would also need to re-consider, without rejecting it, the status of retroactive inhibition theories of immediate forgetting. Common experience indicates the wide variety of the events, often of very short duration, which can damage a recently formed trace. It may be that the process of consolidation of a memory trace is vulnerable to any interruption, not merely to those which introduce, at the time of recall, competitive traces. The problem of defining an interrupting event remains.

I am indebted to Mrs. I. E. Skoulding who carried out all the testing.

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LIKE AND CROSS MODALITY RESPONSES IN NORMAL AND SUBNORMAL CHILDREN

BY

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Experiments which investigated response behaviour in imbeciles and normals, in a conflict situation between a general verbal instruction and direct stimuli, were carried out. It was found that combined verbal-motor responses did not give better results than either verbal or motor responses alone. Results in accordance with the general instruction were significantly better if the subnormal children responded in a different modality from the one in which the signals were given. While normals and imbeciles differed markedly in their ability to perform the tasks in like-modality trials, this difference was less apparent in cross modality responses.

Introduction

Lack of verbal fluency, and difficulties in verbal communication in the subnormal have been pointed out by Tredgold as early as 1908. More recently Luria (1956) and his colleagues in the Soviet Union have suggested that the severely mentally deficient suffer not only from a lack of development of the verbal system, but in addition from a poverty of connection between words and motor behaviour, which is held to be a crucial factor in which the mental processes of defectives of imbecile grade differ from those of normals.

According to this view language, in the normal child, not only serves as a means of communication but also as a mental tool, which develops to direct perception as well as motor behaviour. Speech, whether as instruction or self-instruction according to Luria, modifies and changes the perceptual field making designated features stand out, thus gradually enabling the child to generalize and abstract. Severely defective children on the other hand were shown experimentally to be unable to modify the perceptual field by verbal instruction. Another aspect in normal children's development is the verbal regulation of the inhibition of motor responses. Instructions to the young child tend to provoke general excitation but not to evoke appropriate responses. For example, if the young child is asked to press a rubber bulb and say "yes" to a red light, and to say "no" and not to press to a green one, he will give a pressure response regardless of whether he says "yes" or "no." His own speech acts as an excitatory impulse which can promote but not yet inhibit the response. At this stage his own speech is not regulative. Speech and motor behaviour are as yet not integrated, and the excitatory impulse from his own words sets off the motor response. At three years the normal child who accompanies direct signals by his own speech succeeds in responding discriminatingly and words assume a regulative function. This distinction between regulative function and direct excitation of motor impulses from speech is a central one in Luria's theory.

As a working hypothesis such a view has the advantage that it lends itself more readily to experimentation and remedial training, than one which regards verbal capacity as closely dependent on intelligence as a primary ability. If imbeciles are severely handicapped in using words in their directive function to guide behaviour, experiments should aim to define the conditions under which such impairment might

at least be partly overcome, and which would enable them to compensate for existing deficits. Luria reports that patients suffering from Parkinson's disease could, for instance, overcome their incapacity to continue regular pressure of a rubber bulb if they directed their own activity by counting 1, 2, 3, etc. Similarly, such patients could solve a problem of the kind "how many wheels has a car?" by tapping four times even though they were otherwise unable to tap. In this way words could activate and support an otherwise deficient motor system.

Although the impairment of imbecile behaviour seems to be general, affecting many activities, it seems to be most severe in the verbal system and in those activities which involve coding, classification and the use of symbols. Motor activity can often be substantially improved with suitable training methods. Therefore, it was thought possible that the combined verbal-motor responses of imbeciles might show improvement if compared with verbal behaviour alone. The assumption underlying this hypothesis was that a relatively intact motor system might act as support and reinforcement for verbal behaviour just as the speech system supported motor behaviour in Parkinsonism.

The present investigation was based on observations by Tikhomirova (1956) and Nepomnyashchaya (1956) concerning the rôle of speech and motor behaviour in the severely subnormal. Their studies show that imbecile children, if placed in a "conflict situation," in which responses required by verbal instructions are contradictory to those arising from the impulses set off by direct stimuli, tend to respond to the direct rather than to the verbal signals.

In our experiment the subjects were presented with verbal as well as with non-verbal stimuli. A verbal or a motor response as well as combined verbal-motor responses had to be given. The responses had to be made in accordance with a general verbal instruction, which was in contrast to the impulses received from the direct signals.

Method

Forty imbeciles and 20 normals were matched for mental age. The imbecile group was aged 9 to 15 years, mean C.A. 12 years 6 months, mental ages ranged from 5 to 7 years, Stanford Binet I.Q.s ranged from 31 to 50, mean = 41·5. Only children who had no severe motor handicap, who had some speech and who could count at least up to three were included. The normals were 5 to 7 year-olds from an L.C.C. infant school. Each child was repeatedly told what was required of him and the task was demonstrated. The child was also encouraged to repeat verbally the "rules of the game."

Each group was divided into four subgroups. In subgroups one and two the stimulus was one or two pencil taps, in subgroups three and four the stimulus was verbal counting "one" or "one, two." The responses to be made by each group in each stage of the experiment are shown in Table I. Throughout the experiment the subjects had to respond by making two responses if one signal was given and one response when two signals were given.

TABLE I

		Responses		
Groups	*Stimulus*	*Trials* 1–10	*Trials* 11–30	*Trials* 31–40
1	Tapping	Tap	Tap and Count	Tap
2		Count	Tap and Count	Count
3	Counting	Count	Count and Tap	Count
4		Tap	Count and Tap	Tap

Results

Contrary to expectation neither the modality in which the stimuli were given nor the modality in which responses were made had any consistent influence on the results. Significant differences in the ability of the imbeciles to follow a general verbal instruction and to ignore a direct stimulus were, however, evident between those groups who had to respond in the same way in which the stimulus was given, and those who used a different mode of response. If they had to respond with the opposite number of taps to those given by the experimenter, the subjects were unable to do this. Those, however, who were asked to count aloud in opposition to the number of tapping stimuli they received, obtained better results. Similarly, those subjects who were asked to tap in response to the experimenter's counting, did better than those who had to respond by counting aloud themselves. No such differences were found with the normals. Mean frequencies of correct responses for any five trials are given in Table II. As can be seen from this Table, normals respond with a similar number of correct responses under any of the four conditions.

TABLE II

Mean Number of Correct "Opposite" Responses out of a Maximum of 5

Stimulus	*Trial*	*Responses*									
		1–5	6–10	11–15	16–20	21–25	26–30	31–35	36–40
Tapping		*T*		*T* + *C*		*T* + *C*		*T*	
	Imbeciles	1·9	2·8	2·8	1·6	2·5	2·0	2·2	1·6
	Normals	4·4	5·0	4·2	4·8	4·8	4·8	4·6	5·0
		C		*T* + *C*		*T* + *C*		*C*	
	Imbeciles	3·4	2·2	3·1	2·0	1·9	2·2	3·7	2·8
	Normals	3·8	4·2	4·4	4·8	4·2	4·0	4·2	4·0
Counting		*C*		*C* + *T*		*C* + *T*		*C*	
	Imbeciles	2·2	1·3	2·8	1·4	2·2	1·5	1·4	1·6
	Normals	4·6	4·6	3·8	3·8	4·4	4·6	4·2	4·6
		T		*C* + *T*		*C* + *T*		*T*	
	Imbeciles	3·8	2·8	3·7	2·3	2·9	2·7	3·6	2·3
	Normals	4·2	3·8	2·0	4·0	4·0	3·8	3·6	3·8

Out of an optimal number of 40, the imbeciles gave 22·7 correct cross modality and 15·9 like modality responses. For the normals the corresponding scores were 31·4 and 36·1. Analysis of variance of the imbecile data showed that like and cross modality responses differ ($p = 0{\cdot}001$). The difference of like and cross modality in normals was not significant. Comparing the frequency of correct opposite responses of imbeciles and normals, a highly significant difference in the like-modality trials was found, while the inferior performance of the imbeciles in the cross modality groups was less marked. There were no differences in any of the groups between the ten responses preceding and the ten following the 20 combined verbal-motor response trials, nor were the combined responses significantly different from those given in only one modality.

Instruction by the experimenter that the response should be made in "opposition" to the direct signals was repeated after each set of ten trials. If the position of correct responses in any series of ten trials is examined, it becomes evident that these occur with relatively high frequency in the imbecile groups soon after the instruction

is given. This instruction, however, gradually tends to lose its effectiveness and the difference between the number of correct "opposition responses" in the first and second half of the series of trials following instruction is statistically significant. No such difference was apparent with the normals.

Luria's observation that little connection between speech and motor behaviour is evident in imbecile subjects was confirmed in this experiment. If the experimenter gave one pencil tap the subject often gave one tap himself while saying: "one, two." When being told "to make it two taps as well as counting two" he often made one or two correct responses, and then either reverted to his previous behaviour or changed the verbal response in such a way that it reflected his motor behaviour.

It could be objected that such behaviour as was observed in the experiment was a direct function of the rather complex verbal instructions, and that a change in its verbal formulation might result in a change of behaviour of the subjects. A further experiment was therefore carried out, using the same task as previously, but instead of instructing the subject "to do the opposite of what the experimenter was doing," asking him to "play a game of three," that is, add so many pencil taps of his own to the experimenter's that together they would always make three.

Ten subjects were matched in age and I.Q. with the previous group. (Stanford Binet I.Q. 32–50, Mean I.Q. 42, C.A. 9–15, Mean C.A. 12·5). Only children who could count up to three were included.

Pencil tapping was used as stimulus as well as response. No cross modality trials were given. The experimenter explained to the subject that a "game of three" was to be played, and that the subject had to add as many taps of his own to the experimenter's as were needed to make three every time. The experimenter then demonstrated the task and performed it together with the child, guiding his hand if necessary for six trials.

Forty trials of one or two signals presented in random order were then given. The subject had to respond for the first ten trials by tapping, by tapping as well as counting aloud for the next 20 trials, and finally again by tapping alone for the last ten trials. Each correct response was rewarded.

The results proved to be very similar to those obtained in the previous experiment for the same task with a different verbal instruction. As no significant difference between the two experimental procedures was found, the results of the second study will not be presented in detail.

Discussion

The experiments described attempted to evaluate Russian findings that imbeciles were unable to take effective account of a general verbal instruction if this was in conflict with the sensory stimuli they received. A further hypothesis which was tested, stated that if the motor system could be activated to reinforce speech reactions, the weakness in the imbeciles' ability to use words as guides for behaviour might at least be partly overcome. It was expected, therefore, that combined verbal-motor responses while showing no improvement over motor behaviour might show such an improvement if compared with verbal responses alone.

The results show that the latter hypothesis, at least in so far as it concerns the rather complex experimental task, is untenable. A combination of verbal and motor responses gave no better results than either response modality alone when in the same modality as the stimulus. It seems that the use of motor movement to reinforce a speech response does not aid it, even when the motor system may be presumed to be the relatively more intact. The motor responses obtained here, lack the character of perceived voluntary movements and fail to facilitate stable joint activity of motor

and speech behaviour even temporarily. An alternative inference from the results would be that the correct method of supporting a speech function has not been developed in this experiment.

The decisive factor, which enables the subjects to comply in part with the instructions to respond in opposition to the direct signals, seems not to be the modality in which either stimulus or response is given. The tendencies to echolalia and echopraxia are more prominent if stimulus and response belong to the same modality. Cross modality reactions on the other hand seem to make a general instruction more effective, allowing greater mobility of response patterns. If the subject is required to tap when the experimenter is tapping, or to count when he is counting the compelling force of the direct signal is too strong to permit any other than a purely imitative response. The impulses received from the stimuli do not generalize so readily if the subject is asked to respond in a different and not in a similar modality.

The terms used above to describe the results are one of a number of possible alternatives. Kounin (1948), for example, has suggested that the rigidity of the subnormal is due to the tendency to segregate rather than to connect behaviour patterns. Thus, while one stimulus evokes its own response with regularity, this fails to generalize to a similar stimulus. This quality of behaviour, sometimes called "concreteness," is not unconnected with the results of this experiment. However, in this case a set to respond in a different modality partly enables the imbecile to give a different type of response, i.e. follow a general instruction and reduce the tendency to echolalia. The new set appears to relieve the stimulus response situation of its segregated character. What makes this interpretation ambiguous is that in the present experiment we are not concerned with a change in the stimulus situation but a change in manner of responses. The precise relationships of response mobility and shifts in the stimulus situation and its effect on imbecile behaviour will need further investigation.

The second conclusion which seems to follow from the results is that a general verbal instruction gradually loses its effectiveness. If the instruction to respond in opposition to the signals had been repeated with every direct signal given, one would expect a sustained response pattern.

The experiments reported suggest two conclusions which might be valuable in training the severely subnormal:

(1) Imbeciles find difficulty in freeing themselves from the compelling force of direct stimuli if they are required to respond in the same modality in which the stimulus is given. In such a situation they tend to imitate the experimenter's action.

(2) In order to have an effective function in regulating behaviour, verbal instructions probably have to be repeated every time a response is required. A generalized instruction gradually loses effectiveness. This second conclusion reinforces Russian findings reported by Luria (1957), that under such continuous reinforcement differential responses in imbeciles could be produced.

The authors are indebted to Dr. J. M. Crawford, Physician Superintendent of Botleys Park Hospital, and to Miss B. F. E. West, Headmistress of Granard Infants School, who kindly provided facilities for the research reported here.

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APPARATUS

THE REFLECTING SHUTTER PRINCIPLE AND MECHANICAL TACHISTOSCOPES

BY

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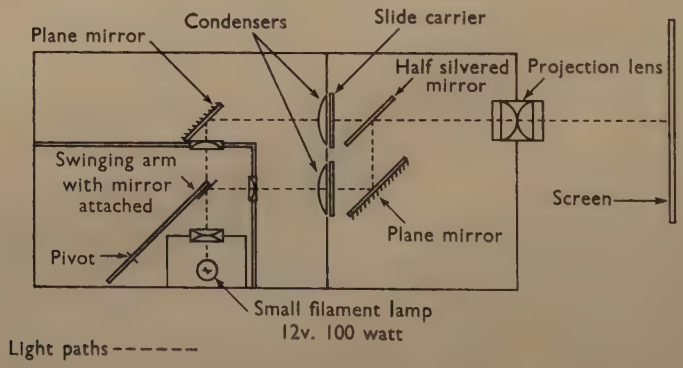
The principle employed in the two tachistoscopes described below is that of using a shutter which reflects light down one channel at the same time as it cuts it off from another. The principle has been applied to produce a high speed projection tachistoscope at a fraction of the cost of a comparable electronic model and a simple direct view classroom tachistoscope. The principle can be further adapted to a variety of uses.

High Speed Projection Tachistoscope

This consists of a light source, as indicated in Figure 1 (of 12 V. 100 watt, actually a car headlamp). The light from this passes through a pair of condenser lenses, forming a small image of the filament. At the point where this image is formed the light is transected by a small mirror. This passes vertically down, being attached to a pivoted arm. The surface of the mirror remains vertical as it passes downwards, and is at an angle of 45° to the light beam. Therefore the light beam takes either of two equivalent optical paths, depending on the position of the mirror shutter. When the mirror shutter is not transecting the beam and diverting it, the light passes through

FIGURE 1



another condenser lens and is then reflected by a fixed mirror at 45°, to pass through a condenser and slide-holder which can be left empty (for it forms the pre- and post-exposure field) or filled with slides to modify the steady field in any way desired. This beam then passes through a half-silvered mirror and then into a projection lens. When the reflecting shutter is in position the light beam is reflected 90° by the shutter to pass into another condenser lens and thence through another condenser and the slide to be exposed. From this the beam passes to a plane mirror set at 45° to be reflected to the other side of the half-silvered mirror and from this to the projection lens.

The model which has been built has a range of exposures from 10 to 250 millisec. This is achieved by a counterweight attached to the periphery of a wheel attached to the arm holding the mirror, where the arm is pivoted. The counterwieght is then slid to one of two positions on the periphery of this wheel. There are also two mirrors of different size which are attachable to the end of the arm to give a coarse adjustment of the range. The fine adjustment of exposure is achieved simply by releasing the dropping catch of the arm from different heights, the height being read off from a calibrated scale. This gravity drive gives a precision of timing and a constancy of operation which is difficult to attain with the type of timing circuit used in electronic tachistoscopes.

Shorter exposure times are attainable by spring-loading the pivoted arm or reducing the size of the mirror and light source. However, it has been found that a 10 millisec exposure with an equally bright pre- and post-exposure field is so low as not to be useful. Visibility at this range of speeds is a function of the brightness of the pre- and post-exposure fields.

The pivoted arm itself is kept from swinging back by coming to rest against a felt lined pad. This pad is placed almost parallel to the motion of the arm, and is forced out of parallel into the path of the motion of the arm by a light spring, so that the arm is progressively slowed down and held by the pad. However, the operator can overcome the force of the spring quite easily by pulling on the arm and reset by completing the revolution of the arm through 360°. Thus the tachistoscope can be reset without re-exposure. The slides which are used are 3 in. square. They can be photographically prepared, but the method which has been found to be most useful is to use gelatine coated glass plates (they are easily prepared) on which material is directly drawn or written in "Photopake" ink. This means that material can be prepared by the experimenter as easily as if it was drawn on paper, once there is a supply of gelatine coated glass plates.

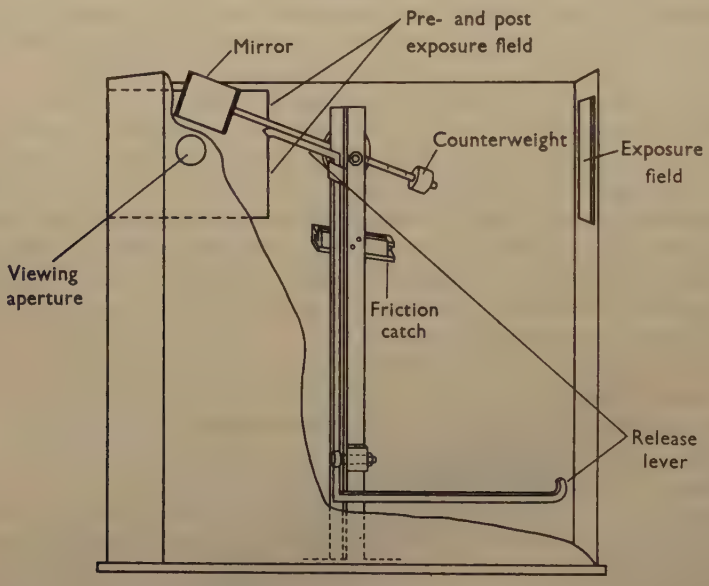
The instrument described has various advantages over the common design of electronic tachistoscopes (other than the obvious engineering ones). It has a larger and more evenly illuminated field, which can be viewed by whole audiences if need be. The spectrum of the light source employed is superior to that emitted by short decay phosphors such as magnesium tungstate, with their bluish-green colour, employed in the electronic device. In spite of the present faith in things electronic, the inherent stability of timing and illumination of the mechanical devices is much better. Where the electronic tachistoscope scores is in the versatility of its operation. It can produce double flashes with dark periods between and so on. But this is not an inherent limitation of the mechanical principle of switching the light beam instead of the light source. By the provision of a sectored wheel, motor driven if necessary such a limitation could be overcome. To produce a dark pre-exposure field, an exposure, a dark field, then another exposure followed by a dark field, a wheel would first have a dark opaque sector then a mirror, then another dark opaque sector, a gap to let the beam through and finally another opaque sector.

A Direct View Tachistoscope

In this application of the reflecting shutter principle, the pivoted arm with the mirror at the end of it (Fig. 2) moves past an aperture very close to the eye. The mirror is at 45° to the sagittal plane. The pre- and post-exposure field is set at the same optical distance as the exposure field, with a cross on it to enable fixation to take place. The two fields are easily equated for brightness. The arm projects beyond the pivot in both directions, to enable a counterweight to be slid on it to control the speed of exposure. The model made at this laboratory has a range of 40 to 120 millisec. using

a single mirror. This mirror measures 1·6 in. in the dimension which travels across the field of vision, and is 2 in. in the other dimension. Further range could be added by allowing the mirror to swing from different heights, using a more extended shaft for the counterweights and using a range of mirrors. However, for practical class use such elaboration is probably not ordinarily warranted. The drawbacks of this tachistoscope are that it is monocular (though a binocular adaptation could be designed) and that it scans the field of view. (In this last respect the projection tachistoscope is good.) However, the effect of scansion is minimized by having a fairly large mirror and that which remains seems of no great importance in classroom experiments (or, indeed, for most research purposes). One advantage which it has is that three-dimensional objects can readily be exposed. It can also be used where a fast switch from one card is desired by dropping the mirror before the aperture and keeping it there by means of a catch.

FIGURE 2



The devices described above have been found to be reliable and free from servicing difficulties, and to compare favourably with electronic devices constructed for the same purpose, for they are—what is above all important in experimenting—understood by the user.

My thanks are due to Mr. R. Shrimpton who was responsible for much of the detailed design and to whom the successful manufacture of the above instruments is due.

PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY SOCIETY, 1959

9th–10th April, 1959. Extended meeting at Oxford.

1st Session: "Diluted Water," by J. A. Deutsch and D. Jones. "Vision after blindness since infancy," by R. L. Gregory and J. Wallace.

2nd Session: "Recognition and recall," by R. Davis and N. S. Sutherland. "Some experiments on anagram solving," by I. M. L. Hunter. "Sequential redundancy and speed," by P. Bertelson (by invitation).

3rd Session: "Curiosity and alternation," by J. A. Deutsch and J. K. Clarkson. "The effect of local cooling on tactile discrimination," by K. A. Provins (by invitation).

4th Session: "The central control of sensory inflow," by G. Dawson (by invitation). "Neurological mechanisms in sensory integration," by J. T. Eayrs (by invitation).

13th–15th July, 1959. Extended meeting at Cambridge.

1st Session: "The effect of auditory and visual stimulation on the skin responses of schizophrenics," by P. Venables. "Problem solving," by A. R. Jonckheere. "Switching time and short-term memory in selective listening," by N. Moray (by invitation). "Two and three channel listening," by A. Taylor (by invitation). "The effect of preparatory set on immediate memory," by D. E. Broadbent.

2nd Session: "A sensitive method for the study of learning and retention," by J. Brown. "Discrepancies in measures of how much is remembered," by J. A. Deutsch. "The information-span of short-term memory," by E. R. F. W. Crossman.

3rd Session: Symposium on Primate Work. Speakers: A. Cowey, C. G. Gross, J. M. Oxbury, L. Weiskrantz.

4th Session: "Some changes in perceptual processes with advancing age," by P. A. Rabbitt (by invitation). "Degree class and attainment in scientific research," by L. Hudson (by invitation). "Some unusual handedness patterns," by O. L. Zangwill. "Experiments on simultaneous contrast," by G. C. Grindley.

5th Session: "Some electrophysiological correlates of attentive behaviour," by G. Horn (by invitation). "Quantum efficiency of rod vision," by H. B. Barlow.

30th September, 1959. One-day meeting at Birkbeck College, London.

1st Session: "The attainment of concepts," by H. Shaffer (by invitation). "Threshold changes for colours induced by anxiety-provoking stimuli," by N. F. Dixon (by invitation).

2nd Session: "Redundancy as an experimental variable," by A. Staniland (by invitation). "The effect of warning strength on the perception of a near threshold stimulus," by A. M. Halliday and R. Mingay.

5th–7th January, 1960. 12th Annual General Meeting at Birkbeck College, London.

1st Session: "Physical determinants of complementary patterns," by J. P. Wilson and D. M. Mackay. "The idea of a psychophysical scale," by C. I. Howarth. "The effect of contrast and spreading on the appearance of colour patterns and structures," by W. D. Wright (by invitation). "The Pickford-Nicolson Anomaloscope," by R. W. Pickford and R. Lakowski.

2nd Session: "Speed of response during massed and spaced learning," by J. Brown and M. Huda. "Michotte's theory of the causal impression," by T. R. Miles (by invitation).

3rd Session: "Syllabic strategies in unconstrained writing," by H. B. G. Thomas (by invitation). "Hesitation and obstruction in speech," by F. Goldman Eisler (by invitation). "Immediate memory," by E. R. F. W. Crossman.

4th Session: " 'Problem-seeking' behaviour in rats," by R. Bambridge and R. J. Audley. "A model for individual choice behaviour," by R. J. Audley and R. Pike. "Printing and the comprehension of scientists," by E. C. Poulton.

5th Session: "Cerebral stimulation and exploratory behaviour in rats," by R. Albino (by invitation). "Grooming as a displacement activity in birds," by C. H. Fraser Rowell (by invitation).

Committee, 1960: President: Professor G. C. Drew.

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BOOK REVIEWS

The Brain and Human Behaviour. Research Publications of the Association for Research in Nervous and Mental Disease. Vol. 36. 1958. London. Baillière Tindall & Cox. Pp. xi + 564. 100s.

Biological and Biochemical Bases of Behaviour. Edited by Harry F. Harlow and Clinton N. Woolsey. Madison. University of Wisconsin Press. 1958. Pp. xx + 476. 63s.

In America to-day, the elucidation of behaviour is a central issue. It is a field in which physiologists rub shoulders with psychiatrists, pharmacologists with experimental psychologists. Although the approach is sometimes labelled inter-disciplinary, the whole movement is better regarded as the convergence of a large number of disciplines upon a common focus. In view of what we stand to gain from a multi-disciplinary attack, every psychologist should welcome this invasion of his traditional preserve.

The two volumes under review testify not only to the rapidly growing concern with problems of behaviour but also to the high standard of much of the work to which it has given rise. The first brings together the proceedings of a meeting of the Association for Research in Nervous and Mental Disease held in New York at the end of 1956; the second is a collection of papers presented at a Symposium on Interdisciplinary Research organized in the University of Wisconsin. Both volumes contain material of the highest interest and importance to research workers in physiological psychology—indeed the present reviewer would regard them as indispensable to anyone who really wants to know what is going on in this rapidly developing field.

The Brain and Human Behaviour is mainly concerned with recent electrophysiological (and to a lesser extent neuropharmacological) studies which have relevance to clinical issues, and it is rather inevitable that such a volume should reflect contemporary preoccupations in neurology. Whereas fifteen years ago much space would have been devoted to the frontal lobes, pride of place is nowadays given to the temporal lobe and its subjacent structures. (Indeed seven of the twenty-one chapters deal explicitly with this topic.) Among contributions of special interest to psychologists may be mentioned Mrs. Brenda Milner's most competent study of the psychological defects produced by temporal lobe excision and the report by Dr. R. G. Bickford and others of retrograde amnesia produced by stimulation of the temporal cortex. Although so far observed in two cases only, this latter finding may prove of real theoretical significance.

A contribution of outstanding importance is the long essay by Dr. Denny-Brown and Dr. R. A. Chambers on the parietal lobe and behaviour. Here the authors present a most detailed report of their own ablation studies in the monkey and attempt some comparison with a variety of clinical syndromes. There is, moreover, a genuine attempt to analyse the data in terms of basic physiological conceptions—an attempt as stimulating as it is rare. Although not everyone will agree with Dr. Denny-Brown's contentions (which in any case are not always easy to grasp), this chapter has the makings of a classical contribution to the physiology of the brain.

Among other chapters of particular psychological interest may be mentioned the late Dr. Lashley's introductory survey of cerebral organization and behaviour, in which he returns—alas for the last time—to some of the philosophical implications of Behaviourism first raised by him in 1923; Dr. Jerome Bruner's essay on neural mechanisms in perception, a thoughtful—if not wholly successful—attempt to look at some of the higher aspects of perception in terms of a neurological model; and Dr. Lamar Roberts' review of the problem of cerebral plasticity in relation to speech.

The Wisconsin Symposium lacks the medical slant of *The Brain and Human Behaviour* and is unashamedly concerned with fundamental issues. To the present reviewer, its most encouraging feature is perhaps the extent to which systematic behaviour study has become integrated into the physiological study of the nervous system. (Indeed it is often difficult to guess whether the writer of any given chapter is a physiologist or a psychologist —a state of affairs unimaginable even ten years ago). Another encouraging feature is the revolt against undue rigidity in the design and technique of experiment and the attempt to cultivate a more adventurous outlook. In writing of primate ablation work, for example, Dr. D. R. Meyer states boldly that he cannot accept ". . . the current philosophy that all research must conform to the pattern of test, operate, retest and reconstruct."

He gives interesting examples of the gain to research of a less stereotyped approach. Dr. Hebb goes even further in arguing that the current approach to psychology in terms of "hypothesis testing" is a barrier to research. "We had much better be naïve and productive," he concludes, "than sophisticated, hypercritical and sterile." William James, one feels, would have agreed.

To the present reviewer, contributions to this Symposium of outstanding interest comprise Dr. Karl Pribram's account of neocortical function and behaviour, in which the author shows that he can handle ideas with no less skill and grace than he wields the scalpel, and Dr. Roger Sperry's account of neural plasticity and his own brilliant work on the effects of callosal section. There are also interesting and provocative chapters on neurochemical factors in the regulation of behaviour and on some neurological implications of intracranial "self-stimulation."

Taking the two volumes together, one has the reassuring impression that physiological psychology (by which is meant in this context the study of the brain as the instrument of behaviour) is very much on the move. Curiously, its antecedents lie less in modern neo-Behaviourism than in the older tradition of American "Functionalism," in the work of such men as James, Ladd, J. R. Angell, Loeb, Franz and Coghill. It betrays an approach more realistic, if less precise, than that of Hull or Skinner, and its findings can be more readily assimilated to those of other biological scientists concerned with the mechanisms of the brain. The present reviewer must also confess his unutterable relief to find that there is scarcely a mention of "learning theory" in either volume.

O. L. Zangwill.

Motivation: A Systematic Reinterpretation. By Dalbir Bindra. New York. Ronald Press Co. 1959. Pp. v + 361. $5.50.

Bindra has produced a useful compilation of the facts dealt with under the heading of motivation. His book is not a reinterpretation in any usual sense; it is an interpretation in the conventional language of learning theory and some Hebbian associationism. As the author says (p. 289) "The general point of view adopted in the writing of this book lies somewhere between the positions of Skinner and Hebb." Consequently, Bindra's attempt at a system fares no better than those from which it is derivative. Being however more attentive to the facts than his predecessors, in order to use the same notions, he has to make them more elastic and vague.

For instance, throughout the book, it is the rôle of learning and experience which is stressed. His position is often so general that it is hardly open to empirical disproof. For instance, his generalization I states (p. 64): "Some degree of repeated or continued exposure to a situation is a necessary condition for the development of activities that are goal-directed with respect to some feature of that situation." Mating in rats which Bindra concedes, occurs without learning, is denied the status of a goal-directed act and relegated to the status of a chain reflex. A different way out is taken with Epstein and Stellar's evidence that the taste of salt in adrenalectomized rats is unlearnt. Such animals adjust their salt intake on being allowed access to a salt solution in proportion to their deficit; and their appetite is as much in evidence during the first hour after being allowed access as afterwards. Bindra suggests that this is learnt, but fails to quote Thomson and Porter's (1953) study, discouraging to such a supposition. A part of the difficulty with Bindra's position is that he gives no hard and fast statement of how he conceives learning to take place or to specify what the various learning processes which he invokes might be. As a result, though he demands almost impossible standards of experimental stringency for those who would claim that certain motivational activities were innate, no such standards can be found when we examine his own position. As a result, almost any evidence is good enough for him to show that an activity is dependent on learning or experience, and explanations redolent of the palmy days of muscle-twitchism abound (for instance (p. 109) when he deals with Birch's ruffed female rats). Why such experiments are held to show that behaviour is not unlearnt is difficult to see and this applies to all the rather naïve deprivation experiments quoted by Hebb's supporters. It is difficult simply to deprive an animal of normal experience, because we invariably substitute abnormal experience and this will lead to the learning of interfering modes of adjustment. It is not in dispute that unlearnt behaviour patterns can be disrupted.

Another example of the one-sidedness of Bindra's critical acumen is his examination of the work of the ethologists on the subject of unlearnt behaviour. He quotes "properly controlled tests" of their experiments. An example of these is the work of Hirsch, Lindley and Tolman who "repeated" Tinbergen's work on the fear response to the hawk.

This "repetition" consisted of ignoring all the characteristics of speed, height and size of the hawk-model. Even the size of the retinal projection (assuming a chicken to go on nothing else) of the model would have been more like that of a very large aircraft at the altitude of a hawk. Of course a negative result was obtained.

Similarly, he quotes Riess (1950), but not Eibl-Eibesfeld who shows that Riess's result on deprivation of objects to carry and nest building was due to a disregard of the conditions under which nesting ordinarily takes place.

His inclusion of a chapter on "Arousal and behaviour" is praiseworthy, comprising as it does a discussion on the rôle of the reticular activating system. However, some of his interpretations of particular experiments seem puzzling. Thus, on p. 232 we find: "Tinbergen (1951, p. 29) has reported a study which suggests that the newly hatched chicks of the herring gull become more excited as judged by the frequency of pecking, on the presentation of a model of a herring gull head which has a red dot on the bill." Summing up such interpretations on the same page he states: "Thus a variety of studies can be interpreted as showing that changes in complex stimulus patterns lead to an increased level of arousal, and that the level of increase brought about by different patterns is different."

It seems a pity that the experiment of Mowrer and Jones (1943) is quoted (p. 170) as having shown that the ease of extinction of a response is a function of the effort required in making a response, after Trotter (1956) has shown that this result was an artefact due to the weighted lever employed in the apparatus in this experiment. It is perhaps less excusable that Hull's classic (1933) experiment is not correctly reported. The author (p. 181) errs in stating that the animals were given forced runs. Further the author states: "On hunger days the animals were led into the food goal-box, and on thirst days into the water goal-box." Hull used only one goal-box. The discussion of the blood chemistry of hunger takes no cognizance of the recent work of such theories as the glucostatic (Mayer) and the lipostatic (Kennedy) and the large amount of work connected with these. Of minor importance are the misprints on p. 8, 9 and 179, where "ethnologists" should read "ethologists." Of greater importance is the point that Freud is placed among those who have used the concept of "instinct" as contrasted with those who use "drive" (p. 6 and 307). Freud's notions about *Trieb* (which has been dubbed "instinct" by his translators and so misled Bindra) are wrong, but they strikingly anticipate the notions of Hull and, when we examine Freud's beliefs in the learnt nature of motivation, those of Bindra himself.

J. A. Deutsch.

Behaviour and Psychological Man. By E. C. Tolman. London. Cambridge University Press (for University of California Press). 1958. Pp. ix + 269. 17s. 6d. net.

This is a collection of nineteen of Tolman's published papers. The first is "A New Formula for Behaviourism" (1922), and the last is "Cognitive Maps in Rats and Men" (1948). These essays are, therefore, drawn from a long period of his work.

Many of the essays are not so much theoretical as metatheoretical, in that they contain mainly comments on the *kind* of theoretical structure which the author favours. In these chapters such problems as the amalgamation of behaviourism and Gestalt Theory are discussed. Empirical observations are here referred to as illustrations, rather than as evidence upon which any theoretical view depends. There are also three or four essays which may be said to be concerned with the metaphysics of behaviourism—with the status of consciousness, of emotion, and so on.

The remainder of the book is composed of two or three papers which may properly be called theoretical. In these chapters, experimental results on latent learning, vicarious trial and error, and related aspects of behaviour are reported. Most of this work is described in greater detail elsewhere. Finally, there is one paper, "The Acquisition of String Pulling by Rats," in which theoretical argument is related in some detail to a specific piece of experimental work.

The papers devoted to conceptual analysis and to general accounts of the nature of psychological theory were, for the most part, written twenty or more years ago. It is difficult now not to find them rather tedious. For, although they compare favourably with many similar works, psychologists have not usually been very successful in discussion of philosophical problems. It may be that this phase was necessary and important in the development of the subject. But it is with some relief that one finds today that such topics are more frequently set aside for analysis by those better qualified for this sort of thing. Further, it may be doubted whether any general specification *can* be given for psychological theories, apart from certain obvious logical requirements. Rather

we must apply the pragmatic test of whether they work in organizing and predicting observed behaviour. It is specially difficult to apply this test to Tolman's theories. But, in the more formal part of his theoretical papers, it is often difficult to see that the organization consists in more than the use of some rather misleading and ill-defined names. And, also in the more formal part of his work, it seems that the theory may predict anything or nothing in any actual experimental situation. The "schematic-sowbug," for instance, remains a creature of very uncertain habits.

This is the unsatisfactory aspect of this group of papers. But this collection is not quite a representative sample of Tolman's work. The book is to some extent redeemed by the less conceptual and more experimental papers which afford a better insight into Tolman's genuine theoretical thinking and into his experimental practice. It should not be forgotten that Tolman is largely responsible for the distinction between learning and performance, and for maintaining this distinction in what were, sometimes, rather adverse circumstances. Again, his insistence upon the basically goal-directed nature of behaviour is clearly expressed in some of these papers. This provides a valuable contrast with the theoretical views of some other authors, from whose systems purposive behaviour emerges more or less as a by-product. Whatever one believes the truth to be in these matters, it is still helpful at the present time to have the issues clearly stated.

In summary, this collection has little to offer to those already familiar with the main points of Tolman's work. They may well feel that this particular sample fails to do full justice to the important aspects of his work. Those who have not previously studied this part of behaviour theory will find selective reading valuable.

A. J. Watson.

Right–Left Discrimination and Finger Localization, Development and Pathology. By Arthur L. Benton. New York. Hoeber-Harper. 1959. Pp. xv + 185. $7.00.

Sooner or later somebody will want to write a book which gives a comprehensive account of the consequences of cerebral lesions in man in terms which are equally applicable to normal behaviour. Such a synthesis might well owe more to studies of brain damaged patients than to studies of normal behaviour, and psychology stands to gain more from the enterprise than clinical neurology. If therefore the title of Professor Benton's book appears narrow in compass it is worth remembering that he takes his cue, not from the theories and experiments of psychologists, but from a famous and puzzling neurological syndrome.

Professor Benton makes three main contributions to an understanding of Gerstmann's syndrome. He presents useful normative and developmental data, he uses methods which can be adapted to clarify and qualify the terms "finger agnosia" and "right–left disorientation" which have been in more or less uncritical use by neurologists since 1924, and he offers a theory that attempts to explain right–left disorientation in terms of impaired language functions.

The normative and developmental studies assembled here were based on normal adults and on normal and intellectually defective children. The adult studies make it clear that defects of finger localization and right–left discrimination on the type of tests used by Gerstmann are rare in normal adults. The studies of children go further and throw useful light on the development of these skills and their degree of association (in children) with other special abilities and with mental age. Some of the findings are of special interest. The most stimulating is perhaps the association observed between strength of hand preference and facility in right–left discrimination.

The most interesting and the most controversial section of this monograph is in the last three chapters which deal with impairment resulting from cerebral injury. Here Professor Benton discusses the neurological literature and attempts to integrate his normative findings with clinical evidence. A description of the various forms of finger agnosia and right–left disorientation and an account of other commonly associated parietal symptoms leads him to quite justifiable conclusions concerning the status of the four Gerstmann symptoms as a syndrome. There is, however, a somewhat unprofitable discussion of the "body schema."

The author's own studies of children and defectives become more obtrusive in his analysis of right–left disorientation and tend to steer his interpretation of the pathological states. The outcome is a somewhat simplified treatment of the issue of cerebral dominance and a more radical version of Head's views on the role of "symbolic formulation." For Head, "symbolic formulation and expression" were (implicitly) antecedent to language, and impairment of this function might result in disabilities (such as the Gerstmann

tetrad) which did not stem from language impairment as such. For Benton, it seems, "symbolic formulation" *is* language and "right–left disorientation of this (bilateral) type is an expression of aphasia." The evidence for this is, as he admits, by no means conclusive. This theory is to some extent the consequence of a tendency to mould the facts and fables of the clinical literature to the reassuringly precise forms yielded by normative studies. The relevance and value of these studies is undoubted, but it can be misleading to assume that correlations between different normal performances correspond to the lines of cleavage of selective intellectual disability following cerebral injury.

Finally, it is worth repeating that Professor Benton's monograph is more ambitious than his unpretentious title suggests. Positive suggestions concerning the psychological background of neurological syndromes are at present inevitably controversial. Such suggestions are, however, valuable and are too rarely uttered by psychologists.

MALCOLM PIERCY.

Sympathectomy. By P. A. G. Monro. London. Oxford University Press. 1959. Pp. xx + 290. 75s. net.

This monograph is the record of 10 years personal research into the anatomy and physiology of the peripheral autonomic nervous system. The material is well presented and there is an extensive bibliography. A valuable feature is an appendix describing in detail the author's experimental methods; in particular the techniques he has developed for plethysmography, automatic skin temperature recording and the determination of the E.S.R.

Dr. Monro displays clearly the complexity of the peripheral organization of autonomic functions and his work challenges several preconceptions. He provides evidence for the existence of many "alternative" autonomic pathways and regards the classification of "grey" and "white" rami communicantes as solely pre- or post-ganglionic as unjustified. Perhaps his most striking conclusion and one that is of great theoretical interest is that recovery of sympathetic functions in a limb following section of its normal preganglionic fibres may be a functional reorganization brought about by "*collateral sprouting* from intact preganglionic fibres."

Dr. Monro's observations are detailed and carefully made and this book will be invaluable to the anatomist and the surgical specialist. It will also be a valuable source book and at times corrective to the research psychologist who measures peripheral autonomic "indices" and treats them as closely correlated with complex central emotional states.

ALICK ELITHORN.

Dynamics of Behavior. By R. S. Woodworth. London. Methuen. 1959. Pp. x + 403. 36s.

In this short book Woodworth provides a supplement to *Experimental Psychology*, elaborated from a course of lectures presenting his own theoretical position. It is mainly devoted to theories of motivation, a discussion of some topics in perception and an account of learning. A number of other topics are dealt with at an elementary level: there are three pages on group behaviour, a paragraph on industrial relations, a page on "insight and invention," a sentence on genetics. The most marked deficiency, for an introduction to psychology, is the very inadequate treatment given to the physiological basis: a chapter on "The Control of Muscular Movement" has an inaccurate half page account of the physiology of muscle, as against three pages on "motion study," and nine on problems of aerial navigation.

In the section on motivation, the theories of Hull, Freud, Murray and McDougall are shortly reviewed. The compression and simplification necessary lead, perhaps inevitably, to some rather misleading formulations. In the discussion on Freud we are told "The Ego generates a drive referred to as 'the safety motive.' " Presumably this is the reality principle. Hull's secondary reinforcement is described as "anticipation of the primary reward." At one point the student is told "we cannot properly say that certain responses become attached to the physiological *need* for food," at another "the act that is reinforced becomes conditioned to the need." Exploration is dealt with at some length, but without mentioning the experiments on alternation.

The chapters on perception deal mainly with distance and orientation, the Gestalt principles and the constancies. The problem of shape recognition is not discussed, though references to the recent papers on this topic by British workers are included in the bibliography.

Woodworth is perhaps at his best on learning. He describes a number of recent experiments simply and clearly, and makes some effective criticisms of the neobehaviourist

account. Only Hull, Tolman and Skinner are dealt with at any length. Guthrie and Hebb are barely mentioned, recent attempts to devise theoretical models of learning are ignored and there is no treatment of memory.

These topics have been selected to illustrate the author's personal account of the dynamics of behaviour. "Dynamic" is not defined. At various points it is applied to "forces or factors" (which are also identified with "intervening variables," and can be introspectively reported on), drives, cause and effect, "the unifying dynamics of question and answer," "situation-and-goal set," and retention. It is mechanistic; in a not untypical argument the ghost of McDougall's vitalism is raised to be once more slain: McDougall believed the individual's choice of goals could not be explained mechanistically. Woodworth replies that natural selection has determined what goals we shall choose, and this is a mechanistic principle.

The main contention is that "behavior consists in active give and take between the organism and the objective environment. This inter-relationship may be called 'dealing with the environment.' " Woodworth proposes a "behavior-primacy theory" of motivation: the "direction of receptive and motor activity toward the environment is the fundamental tendency of animal and human behavior." Play and exploration are not secondarily motivated by organic needs but by the primary drive to deal with the environment. He points out that "need-primacy theory" cannot account adequately for individual adult or play interests, but that his theory does so by postulating "behavioral capacities" and a drive to actualize them.

Perception is described as a process of "decoding stimuli" which "serve as cues of objective facts." This is illustrated by distance perception. Learning is seen largely as a process of becoming acquainted with stimulus sequences, so that the animal "learns to take S_1 as a signal of the coming S_2, and to make preparations accordingly." Conditioned leg flexion to an unavoidable shock is described as a "preparation to 'take' the stimulus." Learning is produced by temporal contiguity and "the unifying dynamics of question and answer." The Law of Effect is replaced by "an undoubted characteristic of behavior which we may call the *ameliorative characteristic.*"

To many psychologists there will be a curiously old-fashioned air about these arguments, for all the good points they contain. Perhaps it is inevitable that an elementary lecture course should contain no elaboration and development of theory, though much restatement. Exploration is indeed a topic of importance when considering motivation. It is perhaps as well to refute the idea that responses are made to isolated "raw stimuli" unaffected by the inter-relationships in the patterns of stimulation received, if this is still held by anyone. And description of behaviour in terms of expectation and preparation is more immediately attractive to the novice than some of the intricacies of S-R theory. But the discussion remains consistently at a verbal level; there is no attempt to analyse or explain the terms by reference to repeatable observations, experimental procedures or any logical or physiological system of structures or relationships. What is a "fundamental tendency of behavior," "objective facts," does "stimulus decoding" say anything more than "afferent neural interaction"? Explicit in Pavlov's work and implicit in much of that of the neo-behaviourists has been the desire to analyse behaviour into units which could be given physiological identification, to provide the basis for a reductionist explanation. Woodworth's failure to appreciate the nature and attraction of this programme probably accounts for the absence of any treatment of modern attempts to devise explanatory models in psychology, and the relative neglect of physiological foundations, in his introductory account.

The peculiarly transatlantic style may grate on a British ear. "A perfect square is more pregnant than any sort of near-square," "how blood chemistry can work on the brain has long been known in regard to respiration," "a social group [consists] of individuals, each of whom responds to stimuli emitted by the others and acts back upon the others," "the cues utilized by the pilot [were] fed into him verbally," "O's motivation is to up his score" are phrases that set the present reviewer's teeth on edge.

There is a useful bibliography containing a number of references to British work.

MICHEL TREISMAN.

Correction: In our review of *Psychopathology: A Source Book*, edited by C. F. Reed, R. E. Alexander and S. S. Tomkins (Harvard University Press) published in this *Journal*, Vol. XI, page 190, we omitted to state that the volume is distributed in this country by the Oxford University Press. We wish to express our apologies to the Oxford University Press for this omission.

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